P

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From March 3, 1892, *to May* 19, 1892.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

March 3, 1892.

Mr. JOHN EVANS, D.C.L., LL.D., Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates for election into the Society were announced, as follows:—

Armstrong, Robert Young, Lieut.-Col.
Beddard, Frank Evers, M.A.
Beevor, Charles Edward, M.D.
Blake, Rev. John Frederick, M.A.
Boulenger, George Albert.
Brennand, William.
Buzzard, Thomas, M.D.
Callendar, Hugh Longbourne.
Davis, James William, F.G.S.
Dibdin, W. J., F.C.S.
Dreschfeld, Professor Julius, M.D.
Dresser, Henry Eales, F.L.S.
Dunstan, Professor Wyndham R.
Eaton, Rev. Alfred Edwin, M.A.
Ellis, William, F.R.A.S.
Etheridge, Robert, F.G.S.
Ewart, Professor J. Cossar, M.D.
Fleming, Professor John Ambrose, M.A.
Foster, Professor Clement Le Neve, D.Sc.
Gadow, Hans, M.A.
Giffen, Robert, LL.D.
Gotch, Francis, M.R.C.S.
Harker, Alfred, M.A.
Hendley, Thomas Holbein, Surgeon Major.
Herdman, Professor William Abbott, D.Sc.
Hill, Professor M. J. M., M.A.
Hinde, George Jennings, Ph.D.
Howorth, Henry Hoyle.
Hutton, Frederick Wollaston, Capt. R.E.
Joly, John, M.A.
Jones, Professor John Viriamu, M.A.
Kidston, Robert, F.G.S.
King, George.
Larmor, Joseph, D.Sc.
Love, Augustus Edward Hough, M.A.
McConnell, James Frederick Parry, Surgeon-Major, F.R.C.P.
MacMunn, Charles, M.D.
Martin, John Biddulph, M.A.

Matthey, Edward, F.C.S.
Miall, Professor Louis C.
Newton, Edwin Tully, F.G.S.
Notter, James Lane, Surgeon-Lieut.-Col.
Oliver, John Ryder, Major-General R.A.
Peach, Benjamin Neve, F.G.S.
Pedler, Professor Alexander, F.C.S.
Reade, Thomas Mellard, F.G.S.
Roberts, Ralph A., M.A.
Rutley, Frank, F.G.S.
Sankey, M. H. P. R., Capt. R.E.
Saunders, Howard, F.Z.S.
Seebohm, Henry, F.L.S.
Sherrington, Charles Scott, M.B.
Smith, Rev. Frederick John, M.A.
Stebbing, Rev. Thomas Roscoe Rede, M.A.
Stevenson, Thomas, M.D.
Stirling, Edward C., M.D.
Tuke, Daniel Hack, M.D.
Ulrich, Professor George Henry Frederic, F.G.S.
Veley, Victor Hubert, M.A.
Waller, Augustus D., M.D.
Waterhouse, James, Colonel.
Woodward, Horace Bolingbroke, F.G.S.
Worthington, Professor Arthur Mason, M.A.
Young, Professor Sydney, D.Sc.

The Right Hon. Spencer Compton Cavendish, Duke of Devonshire, a Member of Her Majesty's Most Honourable Privy Council, whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "Certain Correlated Variations in *Crangon vulgaris*." By W. F. R. Weldon, M.A., F.R.S., Fellow of St. John's College, Cambridge, Professor of Zoology in University College, London. Received February 11, 1892.

The first successful attempt to find a constant relation between the variations in size exhibited by one organ of an animal body and those occurring in other organs was made some three years ago by Mr. Galton; and in a paper read before the Royal Society ('Roy. Soc. Proc.,' vol. 45, p. 135) he determined this relation between several organs of the human body. In what follows an attempt is made to apply Mr. Galton's method to the measurement of the correlation between four organs of the common shrimp. Before the details of the measurement are discussed, a short summary of the method will be given.

Galton's starting point was the fact that each organ of a given race of men varies about its mean size to an extent and with a frequency indicated by the probability equation $\left(y = \frac{A}{\sqrt{\pi}.c}\, e^{-x^2/c^2}\right)$. If two variable organs are known to vary in this way, and if they are so

connected that when the deviation of one variable from its average is known the mean deviation of the second is known, then, evidently, a surface can, from these data, be constructed, showing the relative frequency of occurrence of all possible combinations between the two variables. The changes produced in such a surface by changes in the degree of interdependence of the two variables have been investigated, at Mr. Galton's request, by Mr. J. D. H. Dickson ('Roy. Soc. Proc.,' vol. 40, 1886, p. 63). The results of this investigation, which are of importance for the present purpose, are two :—

(1.) In the population examined, let all those individuals be chosen in which a certain organ, A, differs from its average size by a fixed amount, Y; then, in these individuals, let the deviations of a second organ, B, from its average be measured. The various individuals will exhibit deviations of B equal to x_1, x_2, x_3, ..., whose mean may be called x_m. The ratio x_m/Y will be constant for all values of Y.

In the same way, suppose those individuals are chosen in which the organ B has a constant deviation, X; then, in these individuals, y_m, the mean deviation of the organ A, will have the same ratio to X, whatever may be the value of X.

(2.) The ratios x_m/Y and y_m/X are connected by an interesting relation. Let Q_a represent the probable error of distribution of the organ A about its average, and Q_b that of the organ B; then—

$$\frac{y_m/\text{X}}{x_m/\text{Y}} = \frac{\text{Q}^2_a}{\text{Q}^2_b}; \quad \text{or} \quad \frac{x_m/\text{Q}_b}{\text{Y}/\text{Q}_a} = \frac{y_m/\text{Q}_a}{\text{X}/\text{Q}_b} = r, \text{ a constant.}$$

So that by taking a fixed deviation of *either* organ, expressed in terms of its probable error, and by expressing the mean associated deviation of the second organ in terms of its probable error, a ratio may be determined, whose value becomes ± 1 when a change in either organ involves an equal change in the other, and 0 when the two organs are quite independent. This constant, therefore, measures the "degree of correlation" between the two organs.

A determination of this constant will now be made in the case of five pairs of organs of the shrimp. In accordance with Mr. Galton's notation, the constant will be denoted by r, the mean size of each organ by M, and the probable error of distribution about the mean by Q.

The organs measured are shown in the woodcut fig. 1; they are :—

(1.) The total carapace length, measured in a straight line;
(2.) The length of that portion of the carapace which lies behind the single gastric spine;
(3.) The length of the sixth abdominal tergum;
(4.) The length of the telson.

The measurements made were recorded to within 0·05 mm., and

FIG. 1.

are expressed in terms of the body length, taken as 1,000. As the average length of the shrimps used was rather over 50 mm., the measurements, in the form in which they are recorded, are accurate only to the nearest unit. The roughness of the edges of the parts measured, and the fact that the animals were preserved in spirit, made any attempt to attain greater accuracy exceedingly difficult.

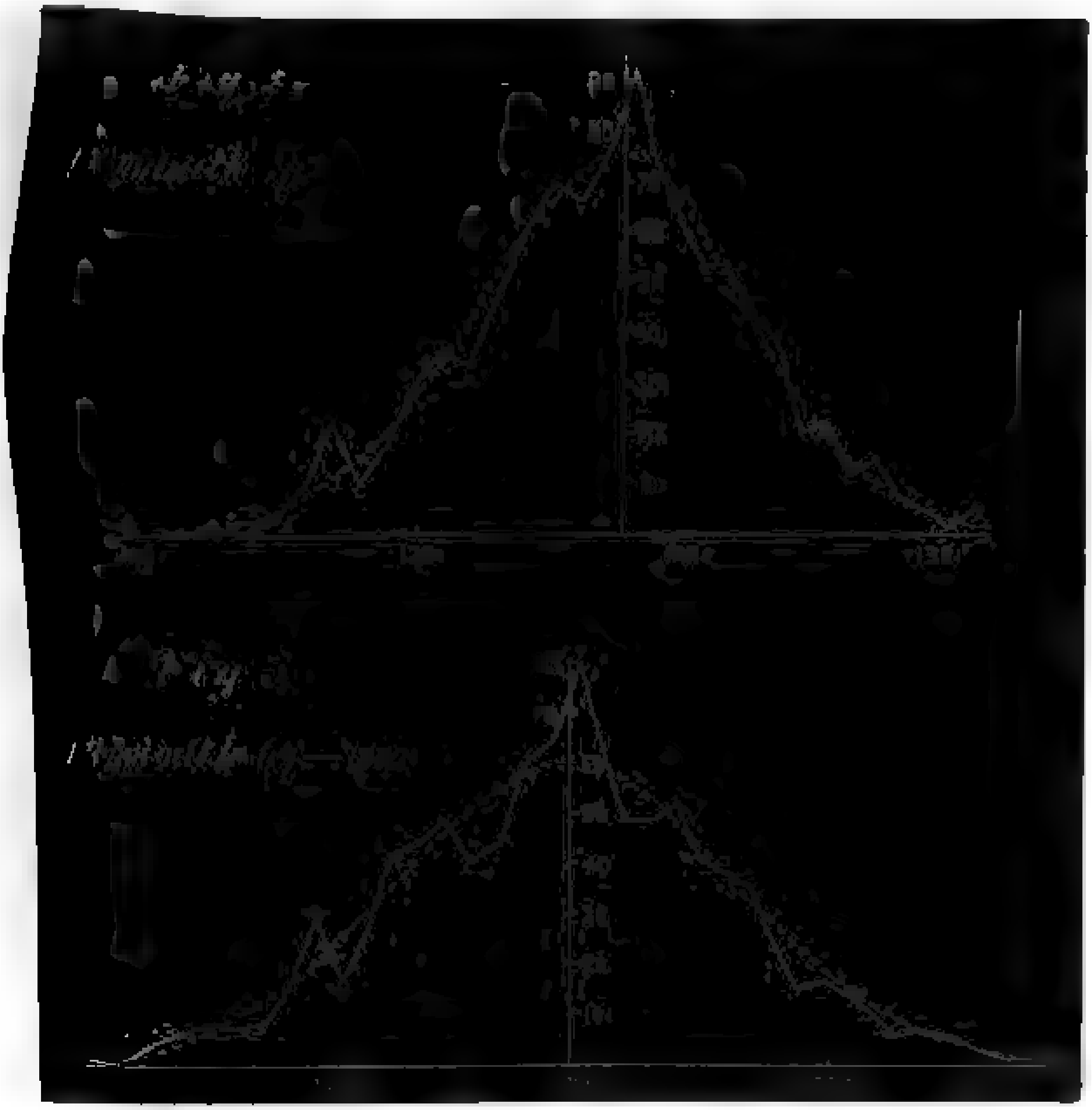
The variations in the length of the four organs here discussed have already been shown to occur with a frequency which agrees very closely with that indicated by the law of probability (*cf.* 'Roy. Soc. Proc.,' vol. 47, p. 445). The closeness with which the distribution of deviations in the samples used agreed with that indicated by a probability curve may be gathered from the diagrams, figs. 2 and 3, which are fairly typical of the whole series.

1.—*Total Length of Carapace and Length of Post-spinous Portion.*

The relation between these two parts has been determined in five races of shrimps; of these the sample containing the greatest number of individuals was obtained at the laboratory of the Marine Biological Association at Plymouth.

The mean length of the carapace, in 1000 adult female shrimps from Plymouth, was 249·63 thousandths of the body length; the probable error of distribution about this average was 4·55. These numbers will be denoted in what follows by M_c and Q_c respectively. The mean length of the post-spinous portion (M_{ps}) was 177·53, its probable error (Q_{ps}) being 3·50.

These numbers having been determined, the individuals were sorted into groups, such that the recorded length of the carapace, *c*, was the same in each group. The mean length of the post-spinous portion, *ps*, was then determined in each group. The results are entered in the first two columns of Table I. Each pair of entries in the table gives a datum for determining the mean deviation of *ps* which is associated with a given value of *c*. If each value obtained in this way be divided by the probable error of the organ to which it belongs, a series of pairs of values will be obtained, from each of which



Figs. 2 and 3.—Curves of frequency of all observed magnitudes of post-spinous lengths (fig. 2) and total carapace lengths (fig. 3) in 1000 adult female shrimps from Plymouth. The dotted curve is a probability curve of the calculated probable error: the continuous curve shows the results of observation. The horizontal scale represents thousandths of the body length: the vertical scale represents tens of individuals.

the value of r can be separately determined. This process has been performed for each of the values entered in the first two columns of Table I, and the results are recorded in the third and fourth columns.

In Table II, the results of a similar process are shown, but the individuals have been sorted into groups in each of which the length of the post-spinous portion of the carapace is constant, and the mean value of the total carapace length has been determined in each group. As before, the immediate results of these determinations are entered in the first two columns, and the deviation of each entry in these columns from its corresponding average, divided by its "probable error," is given in the third and fourth columns.

Table I.—Mean Value of Post-spinous Carapace Length (*ps.*) for every observed Value of Total Carapace Length (*c*) in Plymouth. (1000 individuals.)

$M_c = 249 \cdot 63$; $Q_c = 4 \cdot 55$ $M_{ps} = 177 \cdot 53$; $Q_{ps} = 3 \cdot 50$

Length of *c*.	Mean associated length, *ps*.	$\frac{c - M_c}{Q_c}$.	$\frac{ps - M_{ps}}{Q_{ps}}$.	Length of *ps* when $r = 0{\cdot}81$.	Difference observed, *ps*.
Over 260	188·41	+2·95	+3·11	185·87	+2·54
260	185·41	+2·28	+2·29	184·00	+1·41
259	183·25	+2·06	+1·63	183·38	−0·13
258	182·25	+1·84	+1·35	182·76	−0·51
257	182·34	+1·62	+1·37	182·13	+0·21
256	182·22	+1·40	+1·31	181·51	+0·71
255	181·14	+1·18	+1·03	180·88	+0·26
254	179·98	+0·96	+0·70	180·26	−0·28
253	179·50	+0·74	+0·56	179·63	−0·13
252	179·17	+0·52	+0·47	179·01	+0·16
251	178·68	+0·30	+0·33	178·38	+0·30
250	177·71	+0·08	+0·05	177·76	−0·05
249	177·39	−0·13	−0·05	177·26	+0·10
248	176·64	−0·36	−0·25	176·01	+0·53
247	175·36	−0·58	−0·62	175·88	−0·52
246	175·20	−0·80	−0·67	175·26	−0·06
245	173·56	−1·01	−1·13	174·66	−1·10
244	173·31	−1·24	−1·21	174·01	−0·70
243	173·33	−1·46	−1·20	173·38	−0·05
242	172·81	−1·68	−1·35	172·76	+0·05
241	171·30	−1·90	−1·78	172·13	+0·83
240	169·57	−2·12	−2·27	171·51	+1·06
Under 240	170·33	−2·81	−2·20	169·55	−0·78

The mean value of r, as determined from these two tables, is 0·81; that is to say, if all those individuals be chosen in which the deviation of one of the two organs from its average length is m times the probable error of that organ, the mean deviation from the average size of the other organ in the individuals chosen will be $0{\cdot}81m$ times its probable error.

If the variations in size occurred in each of these two organs with a frequency equal to that indicated by a probability curve, then every pair of entries in each table should give the same value of r. As the conformity between the observed frequency and that indicated by probability is only approximate, the individual determinations give values which differ slightly from one another; but these variations are on the whole small, as may be shown in three ways.

In fig. 4, all the determinations of r are indicated: the deviation of the organ whose value is fixed in each case is measured parallel to the axis of y, the mean deviations of the associated organ parallel to the axis of x: so that each pair of values in Tables I and II determines

Table II.—Mean Value of Total Carapace Length (c) for every observed Length of Post-spinous Portion (ps) in Plymouth. (1000 individuals.)

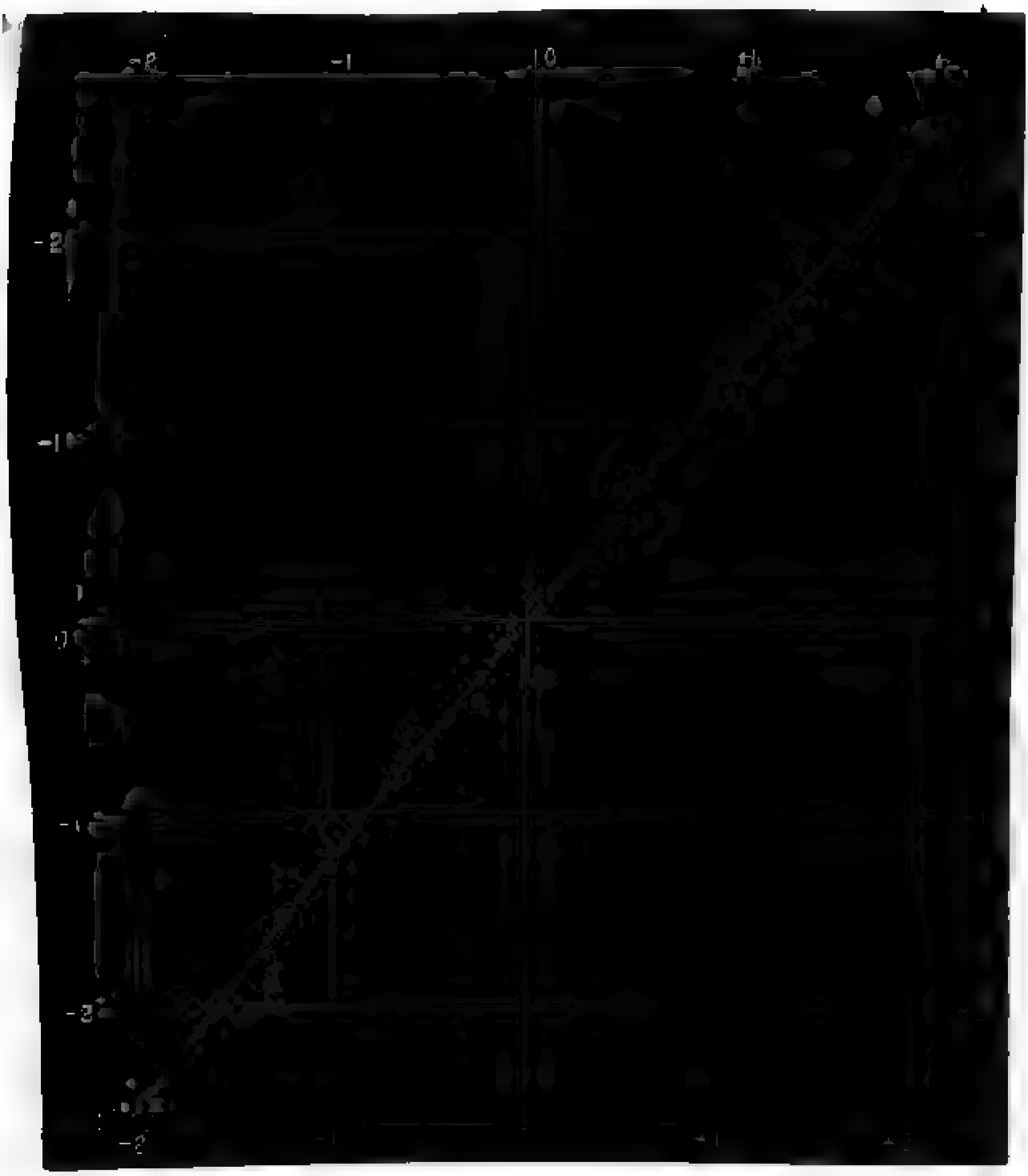
Length of ps.	Mean associated length of c.	$\frac{ps-M_{ps}}{Q_{ps}}$	$\frac{c-M_c}{Q_c}$.	Length of c when $r = 0.81$.	Difference observed, c.
Over 186	262·11	+3·43	+2·74	262·27	−0·16
186	258·25	+2·41	+1·89	258·51	−0·26
185	256·15	+2·13	+1·43	257·48	−1·33
184	256·84	+1·85	+1·59	256·45	+0·39
183	254·88	+1·56	+1·15	255·38	−0·50
182	254·18	+1·28	+1·00	254·36	−0·18
181	253·28	+0·99	+0·80	253·25	+0·03
180	251·73	+0·71	+0·46	252·25	−0·52
179	251·34	+0·42	+0·38	251·18	+0·16
178	249·78	+0·13	+0·03	250·77	−0·99
177	249·10	−0·15	−0·12	249·08	+0·02
176	248·53	−0·44	−0·24	248·01	+0·52
175	246·79	−0·72	−0·62	246·97	−0·18
174	245·73	−1·01	−0·86	245·90	−0·17
173	245·02	−1·30	−1·01	244·83	+0·19
172	243·89	−1·58	−1·26	243·80	+0·09
171	243·67	−1·87	−1·31	242·73	+0·94
170	241·28	−2·16	−1·84	241·76	−0·48
169	241·06	−2·44	−1·91	240·63	+0·43
Under 169	239·88	−3·22	−2·14	237·75	+2·13

the position of one point in fig. 4. Those points whose position is determined from Table I, taking the carapace length as fixed, are indicated by crosses: those obtained from Table II, taking the post-spinous portion as fixed, are shown by dots surrounded by circles ⊙. The straight line drawn across the diagram indicates the ratio $x = 0.81y$; and every point, determined from either table, should therefore lie upon it. It will be seen that the points do, in fact, lie fairly closely round the line, though few lie actually upon it.

The accuracy of the assigned value of r may be tested in another way. When the value of r is known, it is evidently possible to calculate, from a known deviation of one organ, the mean associated deviation of the other; and in the fifth columns of Tables I and II the mean length of each dependent organ which should be associated with every observed value of the independent organ has been calculated on the assumption that $r = 0.81$. The length calculated in this way is found to agree very fairly well with the observed length, which is recorded in the second column of each table. The difference between the observed and calculated length of each dependent organ is shown in the sixth column.

A third way of checking the value of r is given by the ratio of the probable error of distribution of one organ to that of the other. If

FIG. 4.



$r = 0{\cdot}81$, then a deviation of carapace length equal to mQ_c mm. is associated with a mean deviation of the post-spinous portion equal to $0{\cdot}81\,mQ_{ps}$ mm.; and from the values of Q_c and Q_{ps} given above, the ratio $0{\cdot}81\,mQ_{ps} : mQ_c$ is equal to $0{\cdot}62$. In the same way a deviation of mQ_{ps} mm. in the post-spinous portion of the carapace is associated with a mean deviation of the whole carapace equal to $0{\cdot}81\,mQ_c$ mm.; the ratio $0{\cdot}81\,mQ_c : mQ_{ps}$ being equal to $1{\cdot}05$.

From the fundamental formula given above on p. 3,

$$\frac{0{\cdot}62}{1{\cdot}05} = \frac{Q^2_{ps}}{Q^2_c}.$$

$$\text{Now } \frac{0{\cdot}62}{1{\cdot}05} = 0{\cdot}590; \text{ and } \frac{Q^2_{ps}}{Q^2_c} = \frac{(3{\cdot}5)^2}{(4{\cdot}55)^2} = 0{\cdot}591.$$

So that the assigned value of r fulfils all the required conditions very fairly well.

Having found a relation between the deviation of carapace lengths and that of post-spinous lengths, which is constant for all magnitudes of either organ in one local race, the question at once arises whether this relation is not a specific character of the shrimp, which is constant in all local races. At the beginning of the inquiry Mr. Galton suggested to me that the relation between the two organs indicated by the value of r was of such a kind that r might be expected to have the same value in all races of the same species, and in some cases in groups of species. A determination of the relation between carapace length and post-spinous length, and of other relations which are to be discussed below, has abundantly confirmed Mr. Galton's prediction.

In order to test the constancy of the relation between them, the variations in total length of carapace, and in length of the post-spinous portion, were measured in samples of adult female shrimps from Helder (North Holland), from Southport, from Sheerness, and from Roscoff (Finistère)—that is, from four places fairly distant from Plymouth, and differing from Plymouth and from each other in climatic conditions, in salinity and other characters of the sea water, and in nature of the sea bottom.* Each of these races was found to differ from the others in the average length of the organs measured, and in the probable error of distribution of each organ about its average; but the relation between the two organs, as measured by the value of r, was very fairly constant throughout.

The details of each determination of r are given in Tables III—X, at the end of the paper; they are constructed on precisely the same plan as that used for Tables I and II, and need not therefore be further explained. The values of r deduced from all the tables are—

In Plymouth.....	$r = 0{\cdot}81$	(1000 individuals examined).
In Southport.....	$0{\cdot}85$	(800 ,, ,,).
In Roscoff	$0{\cdot}80$	(500 ,, ,,).
In Sheerness.....	$0{\cdot}85$	(380 ,, ,,).
In Helder	$0{\cdot}83$	(300 ,, ,,).

The approach to identity between these values is very striking. The differences between them are certainly large; but they are not, as it seems to me, larger than the probable error of each determination. The number of individuals employed, even in the races from Plymouth

* I am glad to express my gratitude to Dr. P. P. C. Hoek, to Professor Delage, and to Messrs. W. Garstang and W. H. Shrubsole, for their kindness in procuring for me these samples.

and Southport, is too small to allow of a satisfactory determination of the second decimal place. The reader, who cares to do so, may satisfy himself of this by taking 0·80 or 0·82 as values of r in the Plymouth tables, when he will find the agreement between calculated and observed values of the associated organ to be only slightly less close than in the existing table.

It may, therefore, be fairly said that the values of r obtained by examining five races of shrimps are not inconsistent with the existence of a constant value in all the races examined—a value lying somewhere between 0·80 and 0·85.

So that if the deviation of total carapace length from its average be expressed in terms of its probable error, and if the deviation of the post-spinous portion be in the same way expressed in terms of its probable error, then, when either organ differs from its average by any constant amount, the mean deviation of the other will be a constant fraction of that amount, the fraction being between 0·80 and 0·85.

A similar approximation to constancy has been found to exist in the relation between three other pairs of organs, determined in the samples from Plymouth and from Southport. These organs are so largely independent that the probable error of the determination is much greater than before; and accordingly the irregularities in the results are much greater than in the previous case. The mean values of r are as follows:—

	Carapace length and tergum vi.	Carapace length and telson.	Telson and tergum vi.
Plymouth	0·09	0·18	−0·11
Southport	0·06	0·14	−0·09

These values are found from the tables at the end of the paper, from which it will be evident that the determination is much less reliable than that first discussed.

So far, we have only investigated the mean deviation of each organ which is associated with a known constant deviation of another organ. But since a fixed deviation of one organ does not as a rule involve a fixed deviation of the second, it becomes necessary to inquire how the values of this second organ are distributed about their mean. Mr. Galton points out that the deviations of each dependent organ are distributed about their mean with a probable error of $Q\sqrt{1-r^2}$. So that in Plymouth, for example, those post-spinous carapace lengths which are associated with any fixed total carapace length should be distributed about their mean, with a probable error of $Q_{ps}\sqrt{1-(0\cdot81)^2}$ or $3\cdot5\sqrt{0\cdot34}$, or 2·03 nearly. In the same way, the values of total

carapace length which are associated with a fixed length of the post-spinous portion should be distributed about their mean with a probable error of $4.55\sqrt{0.34}$, or nearly 2·64. Therefore, when the Plymouth shrimps were sorted into groups, such as those used in Table I, such that the carapace length was constant in each, then the post-spinous lengths in each group should have been distributed about the mean given in the table with a probable error of 2·03. The largest of these groups contained only about eighty individuals, so that any accurate determination of this point was out of the question; but a rough estimate of the probable error was made in each group which contained more than forty individuals, and the mean value obtained in this way was 1·96, which is perhaps sufficiently near to 2·03. A similar treatment of the groups in Table II gave 2·59 as the probable error of distribution; and this, again, is not very different from 2·64.

The other samples show a similar rough agreement between the probable error of the dependent organ in each group and that indicated by the value $Q\sqrt{1-r^2}$; but the numbers employed are too small to make a determination of this point worth serious discussion.

It cannot be pretended that the results here given are either sufficiently numerous or sufficiently accurate to serve as a basis for generalisation; but at the same time they seem certainly to suggest a very important conclusion. For if the values of r have really the degree of constancy which has been attributed to them, then by expressing the deviation of every organ examined from its average in terms of the probable error of that organ, the deviation of any one of these organs from its average can be shown to have a definite ratio to the associated deviation of each of the others, which is constant for all the races examined. And since both the organs measured and the samples of shrimps examined were chosen, in the first instance, by chance, any result which holds for all these organs through all these races may be reasonably expected to prove generally true of all organs through the whole species.

That is, the results recorded lead to the hope that, by expressing the deviation of every organ from its average in Mr. Galton's system of units, a series of constants may be determined for any species of animal which will give a numerical measure of the average condition of any number of organs which is associated with a known condition of any one of them. A large series of such specific constants would give an altogether new kind of knowledge of the physiological connexion between the various organs of animals; while a study of those relations which remain constant through large groups of species would give an idea, attainable at present in no other way, of the functional correlations between various organs which have led to the establishment of the great sub-divisions of the animal kingdom.

Table III.—Length of Post-spinous Portion (ps) for every observed Value of Total Carapace Length (c) in Southport. (800 individuals.)

$M_c = 248 \cdot 81$; $Q_c = 3 \cdot 96$. $M_{ps} = 180 \cdot 29$; $Q_{ps} = 3 \cdot 65$.

Length of c.	Mean associated length of ps.	Length of ps when $r = 0 \cdot 85$.	Difference observed, ps.
over 256	188·70	189·43	−0·73
256	186·50	186·30	+0·20
255	185·37	185·36	+0·01
254	185·48	184·61	+0·87
253	183·65	183·83	−0·18
252	182·84	183·08	−0·24
251	182·37	182·33	−0·04
250	181·43	181·58	−0·15
249	181·27	180·83	+0·44
248	179·89	180·05	−0·16
247	178·57	179·30	−0·73
246	177·69	178·55	−0·86
245	178·45	177·77	+0·68
244	176·81	177·02	−0·21
243	176·66	176·27	+0·39
242	176·76	175·49	+1·25
241	174·81	174·74	+0·07
under 241	173·54	173·13	+0·41

Table IV.—Value of Total Carapace Length for every observed Length of Post-spinous Portion of Southport. (800 individuals.)

Length of ps.	Mean associated length of c.	Length of c when $r = 0 \cdot 85$.	Difference observed, c.
over 189	258·38	258·38	0·00
189	254·62	256·36	−1·74
188	255·61	255·42	+0·19
187	254·93	254·51	+0·42
186	252·92	253·57	−0·75
185	252·43	252·66	−0·23
184	250·80	251·75	−0·85
183	250·67	250·80	−0·13
182	250·31	249·89	+0·42
181	249·18	248·95	+0·23
180	247·16	248·04	−0·88
179	247·28	247·13	+0·15
178	247·91	246·91	+1·00
177	244·33	245·28	−0·95
176	243·51	244·33	−0·82
175	243·42	243·33	+0·09
174	241·20	242·48	−1·28
173	241·48	241·57	−0·09
172	241·16	240·66	+0·50
under 172	238·78	240·05	−1·27

Table V.—Mean Length of Post-spinous Portion for every observed Value of Total Carapace Length in Roscoff. (500 individuals).

$M_c = 251 \cdot 51$; $Q_c = 3 \cdot 32$. $M_{ps} = 178 \cdot 00$; $Q_{ps} = 3 \cdot 02$.

Length of *c*.	Mean associated length of *ps*.	Length of *ps* when $r = 0 \cdot 80$.	Difference observed, *ps*.
over 257	184·54	184·75	−0·21
257	181·70	181·99	−0·29
256	181·67	181·27	+0·40
255	180·88	180·54	+0·34
254	179·89	179·81	+0·08
253	178·50	179·18	−0·32
252	177·89	178·36	−0·47
251	178·02	177·64	+0·38
250	177·52	176·91	+0·61
249	176·75	176·45	+0·30
248	176·52	175·46	+1·06
247	174·68	174·71	−0·03
246	173·54	173·98	−0·44
under 246	170·06	169·94	+0·08

Table VI.—Mean Value of Total Carapace Length for every observed Value of Post-spinous Portion. (Roscoff: 500 individuals.)

Length of *ps*.	Mean associated length, *c*.	Length of *c* when $r = 0 \cdot 80$.	Difference observed, *c*.
Over 184	260·34	260·98	−0·64
184	257·45	256·80	+0·65
183	256·28	255·70	+0·58
182	254·57	255·02	−0·45
181	254·69	254·14	+0·45
180	253·69	253·26	+0·43
179	252·29	252·39	−0·10
178	251·09	251·51	−0·42
177	250·02	250·63	−0·61
176	250·07	249·76	+0·31
175	248·82	248·88	−0·06
174	246·92	248·00	−1·08
173	245·94	247·11	−1·17
Under 173	240·76	242·33	−1·57

Table VII.—Mean Post-spinous Length for every observed Value of Total Carapace Length. (Sheerness: 380 individuals.)

$M_c = 247\cdot33$; $Q_c = 3\cdot29$. $M_{ps} = 179\cdot68$; $Q_{ps} = 2\cdot91$.

Length of *c*.	Mean associated length of *ps*.	Value of *ps* when $r = 0\cdot85$.	Difference observed, *ps*.
Over 251	184·52	184·94	−0·42
251	185·38	182·37	+3·01
250	181·07	181·62	−0·55
249	180·48	181·07	−0·59
248	181·19	180·26	+0·93
247	179·03	179·44	+0·41
246	178·69	178·62	+0·07
245	177·96	177·98	−0·02
244	176·73	177·26	−0·53
243	177·29	176·51	+0·78
Under 243	175·53	174·38	+1·15

Table VIII.—Mean Carapace Length for every observed Value of the Post-spinous Portion. (Sheerness: 380 individuals.)

Length of *ps*.	Mean associated length of *c*.	Length of *c* when $r = 0\cdot85$.	Difference observed, *c*.
Over 183	253·55	254·97	−1·42
183	249·29	250·56	−0·67
182	248·50	249·59	−1·09
181	249·21	248·60	+0·61
180	247·70	247·64	+0·04
179	246·06	246·68	−0·62
178	245·93	245·69	+0·24
177	246·29	244·73	+1·56
176	243·00	243·76	−0·76
Under 176	242·75	240·68	+2·07

Table IX.—Mean Length of Post-spinous Portion for every observed Value of Total Carapace Length. (Helder : 300 individuals.)

$M_c = 251{\cdot}88$; $Q_c = 4{\cdot}36$. $M_{ps} = 181{\cdot}67$; $Q_{ps} = 4{\cdot}02$.

Length of *c*.	Mean associated length of *ps*.	Length of *ps* when $r = 0{\cdot}83$.	Difference observed, *ps*.
Over 256	187·98	188·28	−0·30
256	183·85	185·21	−1·36
255	184·09	184·44	−0·36
254	185·08	183·67	+1·41
253	182·56	182·90	−0·34
252	181·48	182·13	−0·65
251	179·83	181·64	−1·81
250	179·87	180·60	−0·73
249	179·42	179·83	−0·41
248	178·37	178·93	−0·56
Under 248	176·11	176·19	+0·08

Table X.—Mean Value of Total Carapace Length for every observed Length of Post-spinous Portion. (Helder: 300 individuals.)

Length of *ps*.	Mean associated length of *c*.	Length of *c* when $r = 0{\cdot}83$.	Difference observed, *c*.
Over 187	259·97	260·57	−0·60
187	254·83	256·19	−1·36
186	255·92	255·18	+0·74
185	255·05	254·38	+0·67
184	252·56	253·48	−0·92
183	253·28	252·57	+0·71
182	251·78	251·66	+0·12
181	250·63	250·77	−0·14
180	249·92	249·86	+0·06
179	247·31	248·99	−1·68
178	248·95	248·09	+0·86
Under 178	247·00	243·89	+3·11

Table XI.—Mean Length of Sixth Abdominal Tergum for each Value of Total Carapace Length. (Plymouth: 1000 individuals.)

$M_c = 249{\cdot}63$; $Q_c = 4{\cdot}55$. $M_{Tvi} = 145{\cdot}71$; $Q_{Tvi} = 2{\cdot}82$.

Length of *c*.	Mean associated length of Tvi.	Tvi when $r = 0{\cdot}09$.	Difference observed, Tvi.
257	146·91	146·12	+0·79
256	146·46	146·06	+0·40
255	145·95	146·00	−0·05
254	145·10	145·95	−0·85
253	145·35	145·89	−0·54
252	146·76	145·84	+0·92
251	145·93	145·78	+0·15
250	145·70	145·73	−0·03
249	145·47	145·68	−0·21
248	146·02	145·62	+0·40
247	145·44	145·56	−0·12
246	146·30	145·51	+0·79
245	145·96	145·46	+0·50
244	144·22	145·40	−1·18
243	146·17	145·34	+0·83
242	145·68	145·29	+0·39

Table XII.—Value of Total Carapace Length for observed Lengths of Sixth Abdominal Tergum. (Plymouth: 1000 individuals.)

Length of Tvi.	Mean associated length of *c*.	Length of *c* when $r = 0{\cdot}09$.	Difference observed, *c*.
151	250·26	250·40	−0·14
150	250·00	250·25	−0·25
149	249·79	250·11	−0·32
148	250·42	249·99	+0·43
147	249·46	249·81	−0·35
146	249·19	249·67	+0·48
145	248·08	249·53	−1·45
144	249·24	249·38	−0·14
143	249·84	249·24	+0·60
142	248·72	249·09	−0·37
141	248·55	248·95	−0·40
140	249·06	248·81	+0·25

Table XIII.—Mean Length of Sixth Abdominal Tergum for observed Values of Total Carapace Length. (Southport: 800 individuals.)

$M_c = 248{\cdot}31$; $Q_c = 3{\cdot}96$. $M_{Tvi} = 143{\cdot}56$; $Q_{Tvi} = 3{\cdot}20$.

Length of *c*.	Mean associated length of Tvi.	Length of Tvi when $r = 0{\cdot}05$.	Difference observed, Tvi.
259	145·57	143·99	+1·58
258	140·75	143·95	−3·20
257	144·25	143·91	+0·34
256	142·74	143·87	−1·13
255	142·84	143·83	−0·99
254	143·92	143·79	+0·13
253	143·41	143·75	−0·34
252	144·54	143·71	+0·83
251	144·97	143·67	+1·30
250	143·26	143·63	−0·37
249	143·23	143·59	−0·36
248	143·56	143·55	+0·01
247	143·67	143·51	+0·16
246	143·90	143·47	+0·43
245	144·16	143·43	+0·73
244	142·57	143·39	−0·82
243	143·88	143·35	+0·33
242	142·48	143·30	−0·82
241	143·41	143·26	+0·15
240	142·33	143·22	−0·89

Table XIV.—Mean Value of Total Carapace Length for observed Lengths of Sixth Abdominal Tergum. (Southport: 800 individuals.)

Length of Tvi.	Mean associated length of *c*.	Length of *c* when $r = 0{\cdot}05$.	Difference observed, *c*.
150	249·15	248·71	+0·34
149	248·74	248·65	+0·09
148	247·44	248·59	−1·05
147	247·53	248·52	−0·99
146	249·47	248·46	+1·01
145	248·71	248·38	+0·33
144	247·40	248·34	−0·94
143	247·76	248·28	−0·52
142	247·35	248·21	−0·86
141	247·26	248·14	−0·88
140	249·14	248·09	+1·05
139	245·51	248·03	−2·52
138	248·69	247·95	+0·74
137	246·81	247·90	−1·09

Table XV.—Mean Length of Telson for observed Values of Total Carapace Length. (Plymouth: 1000 individuals.)

$M_c = 249{\cdot}63$; $Q_c = 4{\cdot}55$. $M_{Te} = 193{\cdot}08$; $Q_{Te} = 4{\cdot}56$.

Length of *c*.	Mean associated length of Te.	Length of Te when $r = 0{\cdot}18$.	Difference observed, Te.
258	193·79	194·59	−0·80
257	194·92	194·41	+0·51
256	195·78	194·23	+1·55
255	196·08	194·05	+2·03
254	196·32	193·87	+8·45
253	193·87	193·69	+0·18
252	194·53	193·51	+1·02
251	194·48	193·32	+1·16
250	192·97	193·15	−0·18
249	193·43	192·95	+0·48
248	194·58	192·79	+1·78
247	192 00	192·51	−0·51
246	192·32	192·33	−0·01
245	191·91	192·15	−0·24
244	192·28	192·07	+0·21
243	190·29	191·89	−1·60

Table XVI.—Value of Total Carapace Length for observed Length of Telson. (Plymouth: 1000 individuals.)

$M_c = 249{\cdot}63$; $Q_c = 4{\cdot}55$. $M_{Te} = 193{\cdot}08$; $Q_{Te} = 4{\cdot}56$.

Length of Te.	Mean associated length of *c*.	*c* when $r = 0{\cdot}18$.	Difference observed, *c*.
199	259·18	250·69	+8·49
198	244·25	250·51	−6·26
197	250·54	250·33	+0·21
196	249·39	250·15	−0·76
195	249·37	249·97	−0·60
194	248·81	249·79	−0·98
193	248·30	249·61	−1·31
192	249·27	249·43	−0·16
191	247·82	249·25	−1·43
190	249·36	249·07	+0·29
189	248·74	248·90	−0·16
188	248·91	248·71	+0·20

Table XVII.—Mean Length of Telson for observed Values of Total Carapace Length. (Southport: 800 individuals.)

$M_c = 248{\cdot}31$; $Q_c = 3{\cdot}96$. $M_{Te} = 195{\cdot}48$; $Q_{Te} = 3{\cdot}71$.

Length of *c*.	Mean associated length of Te.	Length of Te when $r = 0{\cdot}14$.	Difference observed, Te.
256	196·25	196·49	−0·24
255	196·00	196·36	−0·36
254	196·81	196·23	+0·58
253	196·75	196·09	+0·66
252	195·70	195·06	−0·26
251	195·48	195·83	−0·35
250	194·81	195·70	−0·89
249	194·05	195·57	−1·52
248	195·95	195·44	+0·51
247	194·94	195·31	−0·37
246	195·22	195·18	+0·04
245	195·28	195 05	+0·23
244	195·29	194·92	+0·37
243	194·92	194·79	+0·13
242	193·93	194·65	−0·72

Table XVIII.—Mean Value of Total Carapace Length for observed Lengths of Telson. (Southport: 800 individuals.)

Length of Te.	Mean associated length of *c*.	Length of *c* when $r = 0{\cdot}14$.	Difference observed, *c*.
202	247·04	249·27	−2·23
201	250·78	249·13	+1·65
200	247·11	248·98	−1·87
199	248·21	248·83	−0·62
198	249·23	248·68	+0·55
197	247·46	248·53	−1·07
196	248·05	248·39	−0·34
195	247·81	248·26	−0·45
194	247·15	248·12	−0·97
193	248·42	247·97	+0·45
192	248·61	247·82	+0·79
191	248·31	247·67	+0·64
190	244·81	247·52	−2·71
189	247·46	247·37	+0·09
188	246·38	247·22	−0·84
187	248·04	247·07	+0·97

Table XIX.—Mean Length of Sixth Abdominal Tergum for observed Lengths of Telson. (Plymouth : 1000 individuals.)

$M_{Tvi} = 145{\cdot}71$; $Q_{Tvi} = 2{\cdot}82$. $M_{Te} = 193{\cdot}08$; $Q_{Te} = 4{\cdot}55$.

Length of Te.	Mean associated length of Tvi.	Length of Tvi when $r = -0{\cdot}11$.	Difference observed, Tvi.
200	145·69	145·23	+0·46
199	144·40	145·30	−0·90
198	144·58	145·37	−0·79
197	143·54	145·44	−1·90
196	145·87	145·51	+0·36
195	145·29	145·58	−0·29
194	145·48	145·65	−0·17
193	144·91	145·72	−0·81
192	145·17	145·78	−0·61
191	147·50	145·85	+1·65
190	147·24	145·93	+1·31
189	145·83	146·00	−0·17
188	142·54	146·07	−3·53
187	145·53	146·14	−0·61

Table XX.—Mean Length of Telson for observed Lengths of Sixth Abdominal Tergum. (Plymouth : 1000 individuals.)

Length of Tvi.	Mean associated length of Te.	Te when $r = -0{\cdot}11$.	Difference observed, Te.
150	193·92	193·70	+0·22
149	191·60	193·56	−1·96
148	193·42	193·41	+0·01
147	193·22	193·27	−0·05
146	192·99	193·12	−0·13
145	193·94	192·98	+0·96
144	193·13	192·83	+0·30
143	192·66	192·69	−0·03
142	193·24	192·55	+0·69
141	193·24	192·40	+0·84
140	191·02	192·26	−1·24

Table XXI.—Mean Length of Sixth Abdominal Tergum for observed Lengths of Telson. (Southport: 800 individuals.)

$M_{Te} = 195{\cdot}48$; $Q_{Te} = 3{\cdot}71$. $M_{Tvi} = 143{\cdot}56$; $Q_{Tvi} = 3{\cdot}20$.

Length of Te.	Mean associated length of Tvi.	Length of Tvi when $r = -0{\cdot}09$.	Difference observed, Tvi.
204	143·78	142·68	+0·90
203	144·60	142·96	+1·64
202	144·96	143·04	+1·92
201	142·41	143·12	−0·71
200	142·02	143·20	−1·18
199	142·36	143·28	−0·92
198	142·95	143·36	−0·41
197	143·54	143·44	+0·10
196	141·23	143·52	−2·29
195	143·88	143·60	+0·28
194	144·35	143·68	+1·27
193	143·95	143·76	+0·19
192	144·18	143·84	+0·34
191	142·91	144·00	−1·09
190	140·03	144·08	−4·05
189	142·36	144·16	−1·80

Table XXII.—Mean Length of Telson for observed Lengths of Sixth Abdominal Tergum. (Southport: 800 individuals.)

Length of Tvi.	Mean associated length of Te.	Length of Te when $r = -0{\cdot}09$.	Difference observed, Te.
150	194·14	194·81	−0·67
149	193·72	194·92	−1·20
148	196·64	195·02	+1·62
147	195·31	195·12	+0·19
146	196·56	195·23	+1·33
145	195·07	195·33	−0·26
144	195·97	195·44	+0·53
143	194·10	195·54	−1·44
142	195·77	195·64	+0·13
141	193·98	195·75	−1·77
140	194·40	195·85	−1·45
139	195·34	195·95	−0·61
138	195·59	196·06	−0·47
137	196·16	196·16	0·00

II. "An Experimental Investigation of the Nerve Roots which enter into the formation of the Brachial Plexus of the Dog." By J. S. RISIEN RUSSELL, M.B., M.R.C.P. Communicated by Professor VICTOR HORSLEY, F.R.S. Received February 18, 1892.

(From the Physiological Institute of Berlin and the Pathological Laboratory of University College, London).

(Abstract.)

The subject is introduced by an allusion to the attempts that have been made by anatomists to determine the functional relationships between the nerve roots and groups of muscles they supply, in which connexion the work of Krause, Schwalbe, Herringham, and Paterson are cited. A brief reference is made to the observations of Erb, Duchenne, Knie, and Thorburn, after which the author refers to the experimental work that has been done in this field by Müller and Van Deen, Kronenberg, Panizza, Peyer, Krause, Ferrier and Yeo, Bert, Marcacci, and Forgue. The anatomical accounts of the brachial plexus of the dog as given by Ellenberger and Baum, Chauveau and Arloing, and Forgue are quoted, the discrepancies which exist between these different accounts pointed out, and the author's own experiences in this connexion, differing in some points, while agreeing in others, with the descriptions given by these observers, are detailed.

He then proceeds to explain his methods of experimentation, which consisted in:—

1. Observation of the compound movements in the fore limb of the dog by electrical excitation of the peripheral end of the whole of a cervico-brachial nerve root which had been previously exposed and divided.

2. Minute differentiation obtained by electrical excitation of the individual bundles composing such a nerve root.

3. Direct observation (after dissection) of the muscles thrown into action by electrical excitation of the separate nerve roots. As a corollary to this, the question as to whether or no a single bundle of fibres representing a single simple movement in a nerve root ever remains distinct in its course to the muscles it supplies, without inosculating with other nerve fibres, is dealt with. A further point determined is whether, when a muscle receives nerve fibres from more than one cervico-brachial nerve root, both nerve roots supply fibres to one and the same muscle fibre or not.

4. Alteration in the action of the fore limb in progression or in standing, evoked by section of a nerve root or roots.

5. Influence of section of a root or roots in excluding part of a

generalised epileptic spasm induced in the limb by cortical excitation.

6. Differentiation of parts of the nerve roots by the degeneration method, in which connexion an allusion is made to certain results obtained by the author, which do not accord with the Wallerian law of degeneration, and which are in accord with the experiments of Joseph.

From the results of these various methods of experimentation, the author draws the following conclusions:—

I. *Stimulation Experiments.*

1. The compound movement obtained by stimulation of a whole nerve root is a well-coordinated one, depending on the action of a group of muscles in synergic combination, as Ferrier and Yeo showed to be the case in the monkey.

2. This compound effect may be resolved into its component factors when it is found that movements diametrically opposed to each other may be represented in the same nerve root, *e.g.*, flexion and extension.

3. Such single simple movements bear a constant relation to the nerve roots, the same movements being always found in any given root, and thus such movements always bear the same relation to the spinal level; *e.g.*, flexion of the elbow is always represented one root higher than extension of the same joint.

4. Fibres representing a certain movement always preserve the same position in a given nerve root; *e.g.*, extension of the wrist is represented by a bundle of fibres in the upper part of the circumference, while flexion is represented by a bundle of fibres in the lower part of the same root.

5. Each bundle of nerve fibres representing a single simple movement in a nerve root remains distinct in its course to the muscle or muscles producing such a movement, without inosculating with other motor nerve fibres.

6. The group of muscles supplied by any given nerve root occupy both the anterior and posterior surfaces of the limb. In other words, muscles whose unimpeded action would produce one movement are represented in the same root as others whose action would produce a movement diametrically opposite.

7. In such combinations certain muscles are always more extensively represented than others, so that, with a current sufficiently strong to stimulate all the fibres of a nerve root equally, certain muscles predominate in their action over others.

8. The muscles whose action predominates in one root always predominate in that root.

9. If the muscles producing flexion of a certain joint predominate

in their action in one root, those producing extension predominate in another.

10. It is possible, by stimulation of a single bundle of fibres in a nerve root, to produce contraction of a single muscle, and it alone.

11. The same muscle is always represented in more than one nerve root, usually two, and to an unequal extent in these.

12. When the same muscle is represented in two nerve roots, the muscle fibres innervated by one root are not innervated by the other.

II. *Ablation Experiments.*

1. Division of any given nerve root produces paresis of the group of muscles supplied by it.

2. This paresis is only temporary, and soon passes off almost completely.

3. Such division of a nerve root does not result in incoördination of the remaining muscular combinations represented in other nerve roots.

III. *Exclusion of a certain Root or Roots during an Epileptic Spasm in the Limb (the root being divided at the time, and not some time previously).*

1. Division of one or more nerve roots produces alteration of the position of a limb during an epileptic spasm, which altered position depends on the particular muscular combinations that have been thus thrown out of action.

2. No incoordination is produced in the action of the remaining muscular combinations.

3. There is no evidence of overflow of the impulses which ought to travel down the divided root into other channels through the spinal centres, so as to reach the muscles by new paths.

IV. *Degeneration Method.*

1. These experiments confirm the anatomical facts that had been previously ascertained by dissection, as to which nerve roots supply any given nerve with fibres.

2. The degeneration which results in the nerves is not a scattered one, but is localised to distinct bundles of nerve fibres occupying a certain position in the transverse section of the nerve.

3. The Wallerian law of degeneration is found to be erroneous with regard to the degenerations which result on division of a nerve root on the distal side of the intervertebral ganglion; for not only is degeneration found in the peripheral end of such a root, but also in that portion of the sensory root between the ganglion and the spinal cord;

pointing to the probability that there are certain nerve fibres which do not depend on the ganglion for their trophic supply, but derive the same from elsewhere, either the spinal cord at another level, or the periphery.

In conclusion, the author calls special attention to the value of the method of excluding one or more nerve roots during an epileptic spasm, as affording a means of confirming the facts that have been previously observed from stimulation of the nerve roots, and also of ascertaining new facts with regard to them and the plexuses which they form. He further goes on to point out that it supplies a valuable means of studying the manner in which conduction of impulses from the cortex through the nerve roots and plexuses to the muscles takes place; and that it is capable of still wider extension, as if, instead of producing general epilepsy, less powerful stimuli be applied to the centres for different movements, as represented in the motor cortex, it will afford a means of connecting such centres, or parts of these, with the nerve roots to which fibres proceed from these cortical motor centres.

III. "The Influence of the Kidney on Metabolism." By J. ROSE BRADFORD, M.D., D.Sc., Fellow of University College, London, Assistant Professor of Clinical Medicine at University College, Grocer Research Scholar. Communicated by Professor SCHÄFER, F.R.S. Received February 18, 1892.

(From the Physiological Laboratory of University College, London.)

The results described in this preliminary communication were obtained in a series of experiments commenced in June, 1889, and at present still in progress, with the object of elucidating the functions of the kidneys, and to gain an insight into the disturbance produced in the economy by disease of these organs.

Method.—All the experiments were made on dogs, and a complete experiment involves the following stages:—

Firstly.—The animal, after being weighed, is placed in a suitable chamber, and fed on a weighed diet containing a known quantity of nitrogen; the water drunk is also measured. The amount of urine passed is measured, and the quantity of urea and total nitrogen in it determined. Finally, the weight of the fæces and the amount of nitrogen in them are also determined. All the nitrogen determinations were made by means of Kjeldahl's method; the urea was estimated by the hypobromite method. A daily determination of the above factors was made for a period of a week, and a daily average

thus obtained. In the earlier experiments only the quantities of urine and urea passed were determined. On removal from the chamber, the animal is again weighed.

Secondly.—The operation described below is performed on one kidney. After recovery from this, the dog is again placed in the collecting chamber, and the above data again obtained for a week or more.

Thirdly.—The second kidney is removed, and the animal again placed in the collecting chamber, the food and excreta being again determined for a period of a week or more.

Fourthly.—At a variable time after the second operation the animal is killed by bleeding, and the amount of nitrogenous extractives present in the tissues determined.

As regards the operative procedures, there is nothing to remark about the second operation—the kidney is removed in the usual manner by lumbar incision; a few words are necessary in order to describe the first operation. After anæsthetising the animal with chloroform and morphia, the kidney is exposed by a lumbar incision and freed from its connexions. The vessels in the hilus are then compressed with the fingers, the kidney transfixed from before back, and a large wedge of kidney substance, with the apex of the wedge at the pelvis of the kidney, removed from the middle of the organ. The piece removed weighed from 5 to 15 grams in different cases. The very free hæmorrhage is arrested by ligature of the large vessels divided, and by pressure on the cut surface. When all bleeding had been arrested (the vessels in the hilus being of course no longer compressed) the cut surfaces of the kidney were brought together by two or three silk sutures passed in deeply, and by numerous superficial fine horsehair sutures involving only the cortex and capsule of the organ. The abdominal wound was closed and dressed in the usual manner.

Full antiseptic precautions were always used, and morphia was given hypodermically to prolong the narcosis.

Summary of Experiments.—Twenty-three animals survived the first operation: fifteen animals survived both operations.

Thus, eight animals died after the first operation and before the second. The causes of death in these eight were as follows:—

In four cases the animals were accidentally killed with chloroform administered to perform the second operation. In two cases the wound became septic and the animals were killed. In one case death resulted from hæmorrhage on the seventh day, and one dog, to which further reference will be made below, died of asthenia thirty-six days after the first operation.

This communication deals with the results obtained in the fifteen complete experiments. In one, the first, no observations were made on the urine, and in three dogs the observations were incomplete, so

that there remain eleven cases in which observations were made on the urine before and after the operations.

Effects of the First Operation (*i.e.*, the removal of a wedge of the kidney substance).—The shock of the operation passes off in about twenty-four hours, but for two or three days there is some hæmaturia, and the appetite is poor. The temperature of the body remains at its normal height, or there may be slight pyrexia. The dog, however, soon regains its former health, and no permanent ill effects result from the operation in the great majority of cases. In one case (one of the eight incomplete experiments) the animal died thirty-six days after the operation, with considerable wasting and loss of appetite, and nothing was found *post mortem* except extreme atrophy of the kidney operated on. The opposite kidney was healthy and of normal size. The atrophy was very marked, as the following numbers show:—7·6 grams of the left kidney were removed, *post mortem* the remaining fragment of the left kidney weighed only 3·5 grams, and the opposite kidney 18 grams. In this case, the only one where death resulted from the effects of the first operation, although the atrophy was very marked, there was microscopically no evidence of cirrhosis, and no lesions of the renal vessels were discovered. The cause of death is obscure, as the second kidney was not removed.

With this one exception, the first operation failed to produce any serious or permanent ill effects, and the only result noticed was slight emaciation, but this was generally recovered from in a week or two.

A period of from one to six weeks was allowed to elapse between the first and second operations, and during this time the animal was placed in the chamber, and the ingesta and excreta determined. The following table gives the results observed in five cases :—

Table I.

Weight of dog.	Daily quantity of urine.		Daily quantity of urea.		Weight of kidney removed.	Interval between operation and observation.	Loss in weight of dog.
	Before operation.	After operation.	Before operation.	After operation.			
No. 16, 16 lbs.	166 c.c.	200 c.c.	7 grams	10 grams	6 grams	16 days	1·5 lbs. in 36 days
No. 21, 13 lbs.	113 c.c.	98 c.c.	9 grams	8 grams	7 grams	15 days	1 lb. in 28 days
No. 22, 14 lbs.	86 c.c.	156 c.c.	5 grams	7 grams	5 grams	35 days	2 lbs. in 39 days
No. 23, 11 lbs.	115 c.c.	140 c.c.	16 grams	15 grams	6·5 grams	14 days	1 lb. in 34 days
No. 24, 15 lbs.	130 c.c.	128 c.c.	16 grams	14 grams	10 grams	16 days	1 lb. in 21 days

From this tabular statement of the results in five cases it will b seen that, as stated above, the results of the operation are trifling when we consider its severity. In one case, No. 16, the output of urea was increased from 7 to 10 grams per diem. In this case the ingesta were not determined, and the apparent increase may have been due to an increased diet. In No. 22 the diet was the same before and after the operation, but there is an increase in the urine and urea after the operation. This case is quoted because it illustrates the maximum effect produced; in no other case was so great an effect observed. In the other cases the effects on the urine, &c., are so slight as to be well within the limits of experimental errors. The loss in body weight is trifling when compared with that described below as resulting from the second operation. The greatest loss observed was in Dog 22, where the body weight fell from 14 lbs. to 12 lbs. The specific gravity of the urine is not permanently affected by the operation. In the first few days after the operation, whilst there is hæmaturia, the urine is frequently more abundant in quantity and the specific gravity then is temporarily lowered, but this soon passes off, and the urine returns to its normal quantity and density.

The Results following the Second Operation.

The results following the removal of the second kidney differ widely from those described above as following the first operation, in that there are frequently no immediate ill effects, the animal running about, &c., within a few hours of the nephrectomy, and there is but little shock, hæmorrhage, &c., when compared with the first operation. The remote effects, however, are very marked: a widespread disturbance of nutrition ensues, accompanied by extreme wasting, hydruria, and polyuria, and with these a fall in the body temperature and a great increase in the nitrogenous extractions of the tissues, provided a sufficiently large amount of kidney has been removed at the first operation.

In all cases the wound has healed up rapidly and soundly, and in no case has death resulted directly from the operation. Out of the fifteen experiments, the first was killed five days after the second operation, and at that time the animal was in sound health. In four cases, No. 2, No. 11, No. 19, and No. 21, the animals were killed 47 days, 60 days, 14 days, and 30 days respectively after the second operation, and the results observed in them will be described below (*vide* Table IV). In the remaining ten cases the dogs either died of a rapidly progressive asthenia, or else they were killed at a time when they were practically moribund.

The results in four cases out of the ten are given in the following table:—

Table II.

Weight of dog.	Daily quantity of urine.		Daily quantity of urea.		Weight of kidney removed at 1st operation.	Weight of 2nd kidney also removed.	Weight of kidney found *post mortem*.	Amount of kidney removed expressed as a fraction of total kidney weight.
	Before 2nd operation.	After 2nd operation.	Before 2nd operation.	After 2nd operation.				
No. 6, 11 lbs.	127 c.c.	270 c.c.	4·5 grams	10·2 grams	7·4 grams	24·4 grams	10·5 grams	$\frac{3}{4}$
No. 9, 15 lbs.	260 c.c.	450 c.c.	9 grams	16·8 grams	6·7 grams	27 grams	12 grams	$\frac{3}{4}$
No. 12, 19 lbs.	157 c.c.	458 c.c.	8·6 grams	13·6 grams	8 grams	30 grams	12 grams	$\frac{3}{4}$
No. 23, 11 lbs.	115 c.c.	550 c.c.	16 grams	21 grams	6·5 grams	22 grams	10 grams	$\frac{3}{4}$

From this table it will be seen that the second operation is followed by a great increase in the amount of urine excreted, and also by a large increase in the output of urea. The increase in the urinary water, however, is greater than the increase in the urea, although the latter, as seen above, is greatly augmented.

This condition of polyuria is accompanied by great wasting.

Thus the weight of	Dog	No. 6	fell from	11	lbs. to	8	lbs. in	50	days.
,,	,,	No. 9	,,	15	,,	10	,,	18	,,
,,	,,	No. 12	,,	19	,,	12	,,	15	,,
,,	,,	No. 23	,,	11	,,	7	,,	25	,,

This wasting is rapid in its course, and is not materially checked by a liberal diet, when the animal's appetite will admit of it. The appetite frequently fails somewhat, but the animals will eat meat in large quantities to within a short time of their death, although they refuse dog biscuit. There is also great thirst, and this, no doubt, is in close relation with the hydruria. When the polyuria is fully established, the rectal temperature falls, so that ultimately it may be as low as 97° F., or even 95° F., the normal temperature varying between 101° F. and 102° F. This condition of polyuria leads to a more or less rapid death; all the animals in Table II either died, or were killed because moribund, in from two to six weeks after the second operation.

The following table gives the results in the remaining six cases out of the ten rapidly fatal cases:—

Table III.

Weight of dog.	Daily quantity of urine.		Daily quantity of urea.		Weight of kidney removed at 1st operation.	Weight of 2nd kidney also removed.	Weight of kidney found *post mortem*.	Amount of kidney removed expressed as a fraction of total kidney weight.
	Before 2nd operation.	After 2nd operation.	Before 2nd operation.	After 2nd operation.				
No. 3, not observed	not observed	381 c.c.	not observed	11 grams	15 grams	28·4 grams	15 grams	$\frac{3}{4}$
No. 4, 16 lbs.	not observed	230 c.c.	not observed	not observed	10·4 grams	25·5 grams	13·6 grams	$\frac{3}{4}$
No. 5, not observed	225 c.c.	330 c.c.	9 grams	more than 8 grams	6·4 grams	19 grams	5·5 grams	$\frac{5}{6}$
No. 14, 14 lbs.	187 c.c.	393 c.c.	11·6 grams	11·4 grams	8 grams	19·5 grams	8·2 grams	$\frac{3}{4}$
No. 22, 14 lbs.	86 c.c.	150 c.c.	5 grams	5·5 grams	5 grams	18·5 grams	10 grams	$\frac{2}{3}$
No. 28, 13 lbs.	92 c.c.	311 c.c.	9 grams	8 grams	10 grams (about)	23 grams	7·8 grams	$\frac{4}{5}$

In *all* these cases it is seen that the amount of urine excreted after the second operation is very large, and in all the cases where the amount of urine was also determined before the operation it is found that the latter daily quantity is far less. In only four out of the six cases was the daily output of urea investigated both before and after the operation. In Dog No. 5 the urea was apparently decreased in amount, but in reality it must have been greatly increased, as, owing to a mistake and absence from the laboratory, the urines, after the operation, were put aside for several days, and only tested when putrefaction had taken place; hence the real amount was probably far greater than 8 grams. In the other three cases, the output of urea was either slightly diminished or slightly increased; this result is of considerable interest from the fact that these dogs ate little or nothing after the operation. Thus, No. 22 passed 5 grams of urea daily with a diet of 230 grams of dog biscuit; after the operation 5·5 grams of urea were excreted, but no food was taken, the animal refusing to eat the biscuit.

In No. 28, 9 grams of urea were excreted per diem with 100 grams of meat and 100 grams of biscuit daily; after the operation 8 grams of urea, with the ingesta diminished to 40 grams of meat and 20 grams of biscuit daily. Similarly, in No. 14, a liberal allowance of meat and biscuit were given and eaten before the operation, but after only small quantities of meat were eaten, and often none at all. The comparatively small quantity of urea excreted in these cases, when compared to the instances given in Table II, is not dependent upon any inability on the part of the fragment of kidney left to excrete urea. This is well shown by the following observation on No. 22. This dog, as just mentioned, excreted only 5·5 grams of urea per diem with no ingesta; but, on a diet of 200 grams of meat, the daily output of urea rose immediately to an average of 15 grams, and on some days as much as 19 grams were excreted by a fragment of kidney found on post-mortem examination to weigh only 10 grams. Hence, even in the cases where the urea is not absolutely largely increased, it is really increased when we remember that the ingesta are greatly diminished, and that the dog may pass as much, or even more, urea during a whole week with no food as the animal previously passed on a full diet, *e.g.*, No. 22. In all six cases described in Table II the operation was followed by death in from one to four weeks.

It is to be noted that in all the ten cases summarised in Tables II and III, the total amount of kidney substance removed amounted to some three-fourths or more of the total kidney weight, with one exception, where only two-thirds was removed. In all these ten cases there were emaciation, hydruria, and polyuria, absolute or relative.

Table IV.

Weight of dog.	Daily quantity of urine.		Daily quantity of urea.		Weight of kidney removed at 1st operation.	Weight of second kidney, also removed.	Weight of kidney found *post mortem*.	Amount of kidney removed expressed as a fraction of total kidney weight.
	Before 2nd operation.	After 2nd operation.	Before 2nd operation.	After 2nd operation.				
No. 2, 21 lbs.	Not observed	342 c.c.	Not observed	15 grams	10 grams	42 grams	28·5 grams	$\frac{2}{3}$
No. 11, 14 lbs.	132 c.c.	313 c.c.	10 grams	12·5 grams	8 grams	25·4 grams	18 grams	$\frac{2}{3}$
No. 19, 18·5 lbs.	140 c.c.	286 c.c.	9 grams	10 grams	5·7 grams	26·2 grams	14 grams	$\frac{2}{3}$
No. 21, 12 lbs.	113 c.c.	200 c.c.	9 grams	7·5 grams	7 grams	22 grams	16 grams	$\frac{2}{3}$

In the remaining four dogs described in Table IV the amount of kidney removed was slightly less, and, as the table shows, the results are different to those described in the ten rapidly fatal cases.

In none of these cases was the operation fatal, and, as previously mentioned, the animals were killed 47 days, 60 days, 14 days, and 30 days respectively after the second operation. In no case was there any great emaciation, the greatest loss of weight being in No. 11, where the body weight fell from 14 lbs. to 12 lbs. In all cases there was marked hydruria, but the polyuria was slight or absent, notwithstanding the fact that there was no failure of appetite. Thus these four experiments are in great contrast to the other ten, where a larger amount of kidney was removed with a uniformly fatal result. It is clear from these results that the increased flow of urine is not dependent simply upon any increased excretion of urea, since the former may exist without the latter. In no case, however, has an increased excretion of urea been obtained without an increase in the quantity of urine.

In no case amongst the ten fatal ones (where three-fourths of the total kidney weight was removed) has the operation been followed by a diminution in the output of urea, provided the ingesta were not diminished.

We may then form the following conclusion, that when a dog is left with only one-fourth of its total kidney weight, a condition of extreme hydruria invariably results. This hydruria is accompanied, provided the appetite does not fail, by a large increase in the output of urea. Further, that if the ingesta are diminished even to zero, the output of urea remains at the height it reached with a diet sufficient to maintain the weight of the animal when in a normal condition.

That the hydruria, although associated with an increased excretion of urea, is not dependent upon it, is shown not only by the fact mentioned above, that by removal of a smaller amount of kidney hydruria pure and simple is produced, but also by the fact that when both hydruria and polyuria are produced they do not begin simultaneously. In other words, when hydruria and polyuria are both ultimately produced by removal of three-fourths of the total kidney weight, the hydruria *precedes* the polyuria. To illustrate this, it will be sufficient to quote one experiment, *i.e.*, No. 23. After the first operation, when 6·5 grams of the left kidney were removed, the dog passed 140 c.c. of urine containing 15 grams of urea per diem, with a diet of 150 grams of meat. On increasing the food to 200 grams of meat per diem, the urine rose to 212 c.c., containing 17 grams of urea. The second kidney, weighing 22 grams, was then removed. In the week following the operation the ingesta fell to 120 grams, and the urea to 13 grams per diem, the urine rising to 380 c.c. In the second week

the appetite was regained, the ingesta returned to 200 grams, the urea rose to 16 grams, and the urine to 480 c.c. In the third week the ingesta were 140 grams, the urea rose to 21 grams per diem, and the urine to 550 c.c.; the animal was then killed, being weak, the body weight having fallen from 11 lbs. to 7 lbs. This experiment illustrates the two stages the animals pass through, the first one where the normal output of urea is maintained, but the method of its excretion is altered, so that the quantity of urinary water is greatly increased. The second stage is one where the quantity of urine is still further increased, with a more or less sudden increase in the urea, accompanied by emaciation, &c. By the removal of very large quantities of kidney substance these two results are obtained almost together, but even then, for a day or two after the second operation, hydruria only is present. When, however, a smaller quantity of kidney is removed the condition called here the first stage is the only one produced, and this condition of simple hydruria is very permanent, as the experiments quoted in Table IV demonstrate. My observations do not show whether this stage of hydruria can be prolonged indefinitely, but they show that the second stage, polyuria, emaciation, is comparatively sudden in its onset, and rapid in its course.

Character of the Urine.—The urine passed after the second operation contrasts greatly with the normal urine of the dog, inasmuch as it is very pale, abundant, and of low specific gravity, *i.e.*, from 1007 to 1020; whereas the normal urine is dark in colour, and its specific gravity is often as high as 1050 or even 1060, and it is scanty in amount. The urine after the second operation contains neither albumen nor sugar. The percentage of solid matter is of course less than normal, but the total solids are not diminished in amount. The ash also is not diminished; but more detailed observations on these points are at present in progress.

With regard to other symptoms of the disorder produced by the operation, it is to be noted that convulsions are absent. Vomiting is rare; it has only been observed once or twice. Diarrhœa is frequently present towards the end, and small ulcers and sores occur about the lips and feet, possibly of traumatic origin. During the last twenty-four or forty-eight hours of life the flow of urine diminishes greatly, so that usually the animals have been killed whilst the polyuria, &c., was at its height, so as not to vitiate the analysis of the tissues. Thus the final symptoms are great prostration of strength and some drowsiness, together with the great fall in the temperature; the last, however, begins as soon as the polyuria is marked, and hence is present for many days before death.

The aortic blood pressure, measured by connecting the carotid artery to a mercurial manometer, is very high when the marasmic condition of the animal is considered. In three cases observed it

has varied between 95 and 100 mm. Hg, the animal being under the influence of chloroform, that is to say, the blood pressure was as high as it frequently is in normal and healthy chloroformed dogs. This height of the blood pressure is in great contrast to the blood pressure in dogs after double nephrectomy, where even on the third day the blood pressure has sunk to a few millimetres of mercury.

Hence the arterial tension is raised when the animal has but $\frac{1}{3}$rd to $\frac{1}{4}$th of its total kidney weight.

Post-mortem Examination.—The animals are greatly emaciated, but usually some fat remains, especially the omental fat. No marked naked-eye changes have been detected, except a marked excess of cerebro-spinal fluid in the cranial cavity. No obvious change was found in the heart or vessels. The abdominal viscera have been found rather soft and sticky, but no other evidence of septic poisoning or of auto-infection has been found.

The kidney fragment has never been found hypertrophied; more frequently distinctly atrophied, the weight of the fragment found *post mortem plus* the weight of the piece removed being generally less than the weight of the opposite kidney. This is in opposition to the results of a French observer.* He, however, removed at the first operation the entire kidney, and then subsequently removed pieces of the second kidney, which had, as is well known, undergone a so-called compensatory hypertrophy. Under these circumstances Tuffier states that the fragment hypertrophies considerably.

Whether the atrophy observed by me is dependent upon the part of the kidney removed, I trust to elucidate by further observations now in progress.

Nitrogenous Extractives of the Blood and Tissues.—The animals, after being anæsthetised with chloroform, were bled to death. 50 c.c. of blood were placed in an excess of rectified alcohol and 50 grams of muscle, liver, brain were similarly treated after being finely divided. After prolonged extraction, the filtrate is then evaporated to dryness over a water-bath, and the dry residue repeatedly extracted with cold absolute alcohol, usually for some hours. The absolute extract is evaporated to dryness on the water-bath and the residue dissolved in water. The material insoluble in absolute is also dissolved in water, and thus two watery extracts are obtained which may, for simplicity, be called the absolute and rectified extract; these are treated as follows:—half of each is introduced separately into a Dupré urea apparatus, and the amount of nitrogen evolved by decomposition with sodium hypobromite determined. In the remaining half of each extract the total nitrogen present was determined by Kjeldahl's method. In this manner a control is kept on the hypobromite method, since, if such a body as urea is present, the Dupré and

* Tuffier, 'Études expérimentales sur la Chirurgie du Rein,' Paris, 1889.

Kjeldahl determinations should nearly coincide, whereas, if such a body as creatin, &c., is present, the one method would yield twice as much nitrogen as the other. In the present communication only the extractives present in what has been called above the "absolute extract" will be considered, and the results obtained in four experiments are given below. In two cases, No. 19 and No. 21, the removal of two-thirds of the kidney weight had produced simple hydruria, and in the other two cases, No. 23 and No. 28, a more extensive operation had produced polyuria as well. In 23, the polyuria was absolute, in 28 relative, the ingesta being diminished in the latter but not in the former.

It is to be distinctly understood that in the following results the extractive is reckoned on urea, because of its solubility in cold absolute alcohol, and because the amount of nitrogen obtained by the Dupré and Kjeldahl methods respectively practically coincided.

	Blood.	Muscle.	Liver.	Brain.
No. 19	0·065 p.c.	0·030 p.c.	nil	0·04 p.c.
No. 21	0·045 „	0·035 „	0·024 p.c.	0·03 „
No. 23	0·410 „	0·430 „	0·200 „	0·24 „
No. 28	0·360 „	0·300 „	0·200 „	0·22 „

No. 23 was passing an average of 21 grams of urea per diem at the time of death, and No. 28 an average of 8 grams, although in the latter case 10·6 grams of urea were excreted in the last twenty-four hours of life. From these results we see that in the case of dogs suffering from the simple hydruria, the amount of "urea" in the blood and tissues is only slightly above the normal. In the case of the dogs in the second stage, suffering from polyuria, &c., the amount of "urea" in the blood and tissues is enormously increased. Thus, in No. 23 at least twenty times the normal quantity of "urea" was present in the blood, at a time when the animal was still excreting an amount greatly exceeding the normal (*vide* Table II). Hence the excess of extractive matter present in the tissues is not dependent on simple retention, but on increased production.

I trust to consider the extractives soluble in rectified alcohol in a future communication, but they also are largely increased in the tissues, but not in the blood.

The specific gravity of the blood serum is lower than normal, sinking frequently to 1025. The total solids, the proteids, and the ash of the serum are all diminished in amount, the last falling to 0·5 per cent. in many cases.

The disturbance of nutrition with increased production of urea

described above does not follow destruction of the renal plexus, nor does it follow free incision of the kidney with subsequent suturing of the damaged organ. It is a phenomenon closely connected with the removal of *large* quantities of kidney, *i.e.*, half of one kidney and the whole of the second. Inasmuch as the phenomena do not ensue after the first and more severe operation, but only after the second and comparatively trivial operation, it must be concluded that they are more related to the *quantity* of kidney removed than to the shock of the operation, or to any reflex disturbance produced by the operation.

The excess of urea in the muscle over that in the liver and brain might be considered as evidence of its production in the muscles. That this is not necessarily the case is shown by the results of the injection of large quantities of urea into the circulation of normal dogs. The dogs were anæsthetised with chloroform, the ureters ligatured, and the urea then injected into the external jugular. After from one to three hours the animals were killed by bleeding, and the tissues examined, as described above.

Dog's weight.	Amount of urea injected.	Time.	Amount of urea in			
			Blood.	Muscle.	Liver.	Brain.
20 lbs.	10 grams.	2¾ hours.	0·11 p.c.	0·08 p.c.	0·04 p.c.	?
13·5 lbs.	20 grams.	1½ hours.	0·25 p.c.	0·35 p.c.	0·22 p.c.	0·2 p.c.

From these and other observations, we see that the percentage of urea in the muscles is greater than in the case of the liver and brain after intravenous injection, and that it may exceed that of the blood. The smaller percentage in the liver is not dependent upon the excretion of urea through the bile duct, because after ligature of the bile duct the percentage in the liver is still lower than that of the muscles after the intravenous injection of urea. After ligature of both ureters, and after double nephrectomy, the same distribution of urea is found in the tissues.

In all the following cases the animals were killed three days after the operation.

The large quantity of urea present in the muscles under all these different circumstances cannot as yet be regarded as evidence of its direct production in the muscles.

Dog's weight.	Operation.	Blood.	Muscle.	Liver.	Brain.
13 lbs.	Ligature of ureters	0·3 p.c.	0·22 p.c.	0·18 p.c.	?
20 lbs.	Ligature of ureters	0·37 p.c.	0·42 p.c.	0·38 p.c.	0·23 p.c.
12 lbs.	Double nephrectomy	0·34 p.c.	0·32 p.c.	0·18 p.c.	0·17 p.c.

Finally, in the tissues of patients dying from uræmia consequent on cirrhosis of the kidney, very large quantities of urea have been found by me, and here also the percentage in the muscles has been much higher than in the liver. This conclusion is based on the examination of the tissues in fourteen cases.

Presents, March 3, 1892.

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March 10, 1892.

The LORD KELVIN, President, followed by the Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Bakerian Lecture was read by the President on behalf of the Author as follows:—

BAKERIAN LECTURE.—"On the Grand Currents of Atmospheric Circulation." By JAMES THOMSON, LL.D., F.R.S., Emeritus Professor of Civil Engineering and Mechanics in the University of Glasgow. Received March 10, 1892.

[PLATE 1.]

(Abstract.)

In this paper a historical sketch is given of the progress of observational and theoretical researches into the nature and causes of the Trade Winds and other great and persistent currents of atmospheric circulation. Mention is made of the fanciful attempts at explanation by Dr. Martin Lister and by Dr. Garden in papers submitted to the Royal Society a little more than 200 years ago, and which are to be found recorded in the 'Philosophical Transactions.' These papers give evidence of the scanty and crude condition of knowledge and speculation on the subject in the early years of the Royal Society; but yet they may probably have had a beneficial effect in instigating Edmund Halley, the astronomer, to communicate to the Royal Society, in 1686, a paper on the "Trade Winds and Monsoons,"* bringing together a systematized collection of observational results, accompanied by theoretical considerations. That paper constituted an important step in the development of the science of the subject, even though his theory, in one of its most important parts, that which relates to the east to west motion of the trade winds, which he attributed to the diurnal revolution round the equatorial zone of the maximum of accumulation of heating effect, turns out to be fundamentally untenable.

Halley's paper was followed, forty-nine years later, by one more

* 'Phil. Trans.,' No. 183, p. 153.

important still, by George Hadley, submitted to the Royal Society in 1735.* This George Hadley was a brother of the John Hadley who invented the instrument known as Hadley's quadrant. Hadley's paper is entitled, "Concerning the Cause of the General Trade Winds," and it is right here to notice that he applied the name "general trade wind" not merely to those winds of equatorial regions to which the name trade wind is commonly restricted, but used it as including also the westerly winds known to be prevalent in higher latitudes, and which were taken advantage of in trade by mariners on ocean passages from west to east. Thus, his theory has a much wider scope than the title of his paper would now, according to ordinary nomenclature, appear to indicate. Hadley brought into consideration, for the first time, as an essential element towards the formation of a true theory, the inertial and frictional effects resulting in the atmosphere from the rotation of the earth; and we may with confidence judge that, in that paper, he offered to the world a substantially true theory of a large part of the system of atmospheric circulation in its grandest and most dominant conditions.

The paper gives a full account of Hadley's theory, accompanied by explanatory remarks, bringing into special notice its most important features; and the author quotes the concluding passage of Hadley's paper, which he considers, though somewhat vague, and not entirely correct in expression, is to be regarded as suggesting a very notable and important principle, *videlicet*:—That, in respect to the earth's rotation round its axis, the sum of the forward turning-force-influences applied by the winds to the surface of the earth, land and sea included, must be equal to the sum of all the backward turning-force-influences likewise applied to the earth's surface; so that these force-influences may be such as conjointly to produce no acceleration or retardation in the revolution of the earth round its axis.

During a period of more than a century from the promulgation of Hadley's theory there was little, if any, remarkable progress in the development of new speculation regarding the grand or perennial currents of atmospheric circulation. Hadley's theory seems to have lain dormant for a long time in the pages of the 'Philosophical Transactions,' and to have become but little known, even among men of science. Sketches of the theory, more or less complete, were from time to time put forward in encyclopædias and in works on meteorology and navigation, but usually without due appreciation of its meaning and importance, and often without any reference to his name. On the other hand, progress was gradually being made in the bringing together of information concerning the winds, so far as regards the temporary and local disturbances of the atmosphere; and speculations or theories were advanced as to hurricanes, tornadoes, or cyclones.

* 'Phil. Trans.,' vol. 39, No. 437, for April, May, and June, 1735, p. 58.

A short sketch of such progressive researches is given in the paper.

Also through information derived from observational sources, it came gradually to be accepted, as an established fact, that in the latitudes outside the limits of the trade winds—latitudes extending from about 28° or 30° up to far towards the poles—the wind, while prevailing from the west, as had been previously known, prevails also more from the equator towards the pole than from the pole towards the equator, so that to take the case of the northern hemisphere, for simplicity, the winds of our middle latitudes were found prevalently to blow from south of west.

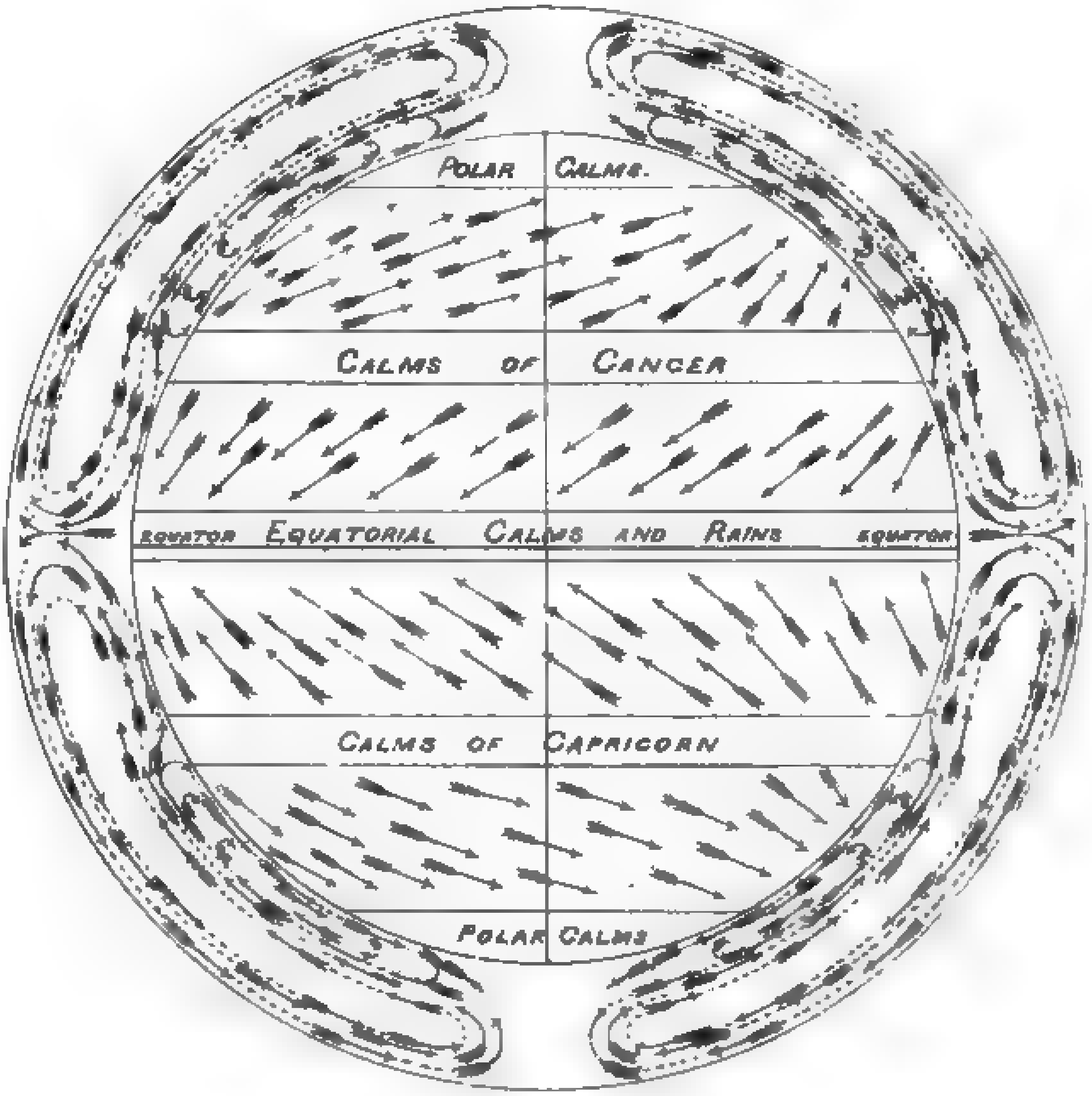
To account for this component from the south in these westerly winds, it came to be very generally supposed among the best writers on the subject that the air departing for the northern hemisphere from the top of the equatorial belt of buoyant air, while flowing northward still in the lofty regions of the atmosphere and over the trade-wind zone; soon becomes a current from the south-west, or from south of west, and continues, after descending to the earth's surface at the northern border of the trade-wind region, still to move forward in continuation of its old course as a current from south of west. But why in the lower regions a poleward motion should be maintained rather than a return flow towards the equator, and how the return from higher to lower latitudes, to compensate for this supposed poleward surface current, should be accomplished, are questions which appear to have been scarcely mooted or to have been left enshrouded in vagueness.

The paper goes on to give an account of the theory which Maury offered in 1855 as an attempt to clear up what he considered "paradoxical" in the theories of others on this subject.

The author of the paper presently reported in abstract (Professor James Thomson), finding Maury's theory untenable, devised a new theory in 1857, and brought it forward at the Dublin meeting of the British Association in that year. In endeavouring to penetrate the mystery as to what the courses of the circulation might be in the middle and higher latitudes, he was, in preliminary ways, fully satisfied that Hadley's theory, in its main features, must be substantially true, and must form the basis of any tenable theory that could be devised. He adopted that theory in all its important features, and superadded further new features, which are told of at length in the paper. His theory, so composed, may be briefly sketched out as follows:—

That at the equator, or near to it, there is a belt of air ascending because of its high temperature and consequent rarefaction:—that its supply of air is maintained by influx from both sides towards the zonal region at its base, which is a region of diminished pressure:—

THOMSON – 1857.



that from its upper part currents float away to both sides, northward and southward :—and that these currents continue in the upper regions of the atmosphere each of them advancing towards, and in part to, the high latitudes of its own hemisphere, until, by cooling, its substance becomes less buoyant, and sinks down gradually in various latitudes of that hemisphere, and forms itself into a return current towards the equator, in the lower part of the atmosphere.

That the air of this great cap of atmosphere, covering the middle and higher latitudes, and including portions of the currents just described, having come from the equatorial regions, which were moving absolutely from west to east in the earth's diurnal rotation with a velocity of about 1000 miles per hour, must, on coming into those new regions much nearer to the earth's axis, have greater velocity from west to east than the earth below it in those new regions has. That in the central or polar part of this great revolving cap of air the barometric pressure must be abated in consequence of the centrifugal tendency due to the extra speed of this great whirling cap of atmosphere. That the bottom layers of this great cap of atmosphere, being by friction on the earth's surface retarded as to this extra velocity of rotation eastward, must have a diminished centrifugal tendency as compared with the quicker revolving air above them, and, consequently, tend to flow, and actually do flow, inwards, towards the region of abated barometric pressure at the centre of the revolving cap of air.

That thus, over the middle, or middle and higher latitudes, there are three currents :—

(1.) A top main current towards the pole.

(2.) A bottom subordinate current towards the pole.

(3.) A middle main current in direction from the pole, and constituting the joint return current for both the preceding currents.

And that all these three have a prevailing motion from west to east, in advance of the earth.

That the great return current, flowing in direction from the pole towards the equator, arrives at a certain part of its course at which it ceases to revolve eastward in advance of the earth; and, for the rest of its course to the foot of the equatorial rising belt, it blows along the surface of the earth as the trade wind of the hemisphere in which it is situated.

The description here given of the author's theory, it is to be noticed, is only a brief sketch. The aërial motions which have been described are illustrated by the accompanying diagram (Plate 1). The arrows on the surface of the hemisphere represent the winds at the surface of land or sea, not the currents in the higher regions. The northward and southward motions, and the up and down motions, in the main currents of the atmosphere, are indicated for all heights in

the cross-section forming the outer part of the diagram. A fuller description, with explanations of reasons for the various statements made, would extend beyond the limits suitable for this abstract.

In the paper the author enters into some considerations as to the reasons for or against the views put forward by various persons.

The paper concludes with a sketch of a contemplated experimental apparatus for illustrating the supposed motions in the earth's atmosphere by motions proposed to be brought into play in water placed in a horizontal circular tray, kept revolving round a vertical axis through its centre, and with heat applied round its circumference at bottom, and cold applied, or cooling allowed to proceed, in and around the central part at or near the surface.

Presents, March 10, 1892.

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Two plaster Medallion Portraits, of Dr. John Richardson, F.R S., and Capt. James Clark Ross, R.N., F.R.S., executed in or about the year 1844. Sir J. D. Hooker, K.C.S.I, F.R.S.

March 17, 1892.

Sir GABRIEL STOKES, Bart., LL.D., Vice-President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Dynamo-Electric Machinery." By J. HOPKINSON, F.R.S., and E. WILSON. Received February 15, 1892.

The following is intended as completion of a paper by Drs. J. and E. Hopkinson ('Phil. Trans.,' 1886, p. 331).* The motive is to verify by experiment theoretical results concerning the effect of the currents in the armature of dynamo machines on the amount and distribution of the magnetic field which were given in that paper, but which were left without verification. For the sake of completeness, part of the work is given over again.

The two dynamos experimented upon were constructed by Messrs. Siemens Brothers and Co., and are identical, as far as it is possible to make them. They are mounted upon a common base plate, their axles being coupled together, and are referred to in this paper respectively as No. 1 and No. 2.

Each dynamo has a single magnetic circuit consisting of two vertical limbs extended at their lower extremities to form the pole-pieces, and having their upper extremities connected by a yoke of rectangular section. Each limb, together with its pole-piece, is formed of a single forging of wrought iron. These forgings, as also that of the yoke, are built up of hammered scrap iron, and afterwards carefully annealed. Gun-metal castings bolted to the base-plate of the machine support the magnets.

The magnetising coils on each limb consist of sixteen layers of copper wire 2 mm. diameter, making a total of 3968 convolutions for each machine. The pole-pieces are bored out to receive the armature, leaving a gap above and below subtending an angle of 68° at the centre of the shaft. The opposing surfaces of the gap are 1·4 cm. deep.

* It must not be supposed from his name not appearing in this short paper that my brother, Dr. E. Hopkinson, had a minor part in the earlier paper. He not only did the most laborious part of the experimental work, but contributed his proper share to whatever there may be of merit in the theoretical part of the paper.—J. H.

The following table gives the leading dimensions of the machine :—

	cm.
Length of magnet limb	66·04
Width of magnet limb	11·43
Breadth of magnet limb	38·10
Length of yoke	38·10
Width of yoke	12·06
Depth of yoke	11·43
Distance between centres of limbs	23·50
Bore of fields	21·21
Depth of pole-piece	20·32
Thickness of gun-metal base	10·80
Width of gap	12·06

The armature core is built up of soft-iron discs, No. 24 B.W.G., which are held between two end plates screwed on the shaft.

The following table gives the leading dimensions of the armature :—

	cm.
Diameter of core	18·41
Diameter of shaft	4·76
Length of core	38·10

The core is wound longitudinally according to the Hefner von Alteneck principle with 208 bars made of copper strip, each 9 mm. deep by 1·8 mm. thick. The commutator is formed of fifty-two hard-drawn copper segments insulated with mica, and the connexions to the armature so made, that the plane of commutation in the commutator is vertical when no current is passing through the armature.

Each dynamo is intended for a normal output of 80 ampères 140 volts, at 880 revolutions per minute. The resistance of the armature measured between opposite bars of the commutator is 0·042 ohm, and of each magnet coil 13·3 ohms.

In the machine, the armature core has a greater cross-section than the magnet cores, and consequently the magnetising force used therein may be neglected. The yoke has the same section as the magnet cores, and is therefore included therein, as is also the pole-piece. The formula connecting the line integral of the magnetising force and the induction takes the short form

$$4\pi nc = 2l_2 \frac{\mathrm{I}}{\mathrm{A}_2} + l_3 f\left(\frac{\nu \mathrm{I}}{\mathrm{A}_3}\right),^*$$

where

n is the number of turns round magnet.

c is the current round magnet in absolute measure.

l_3 the distance from iron of armature to rim of magnet.

* 'Phil. Trans.,' p. 335.

A_2 the corrected area of field.

I the total induction through armature.

l_3 the mean length of lines of magetic force in magnets.

A_3 the area of section of magnets.

ν the ratio of induction in magnets to induction in armature.

f the function which the magnetising force is of the induction in the case of the machine actually taken from Dr. J. Hopkinson on the "Magnetisation of Iron," 'Phil. Trans.,' 1885, figs. 4 and 5, Plate 47.

In estimating A_2 we take the mean of the diameter of the core and of the bore of the magnets 19·8 cm., and the angle subtended by the pole-face 112°, and we add a fringe all round the area of the pole-face equal in width to the distance of the core from the pole-face. This is a wider fringe than was used in the earlier experiments ('Phil. Trans.,' p. 337), because the form of the magnets differs slightly. The area, so estimated, is 906 sq. cm.

l_3 is taken to be 108·8 cm.

A_3 is 435·5 sq. cm.

ν was determined by the ballistic galvanometer to be 1·47. It is to be expected that, as the core is actually greater in area than the magnets, ν will be more nearly constant than in the earlier experiments. It was found to be constant within the limits of errors of observation.

Referring to Diagram No. 1, the curve C is the curve $x = l_3 f\left(\frac{\nu y}{A_3}\right)$, and the straight line B is the curve $x = 2l_2 \frac{y}{A_2}$, whilst the full line D is the characteristic curve of the machine

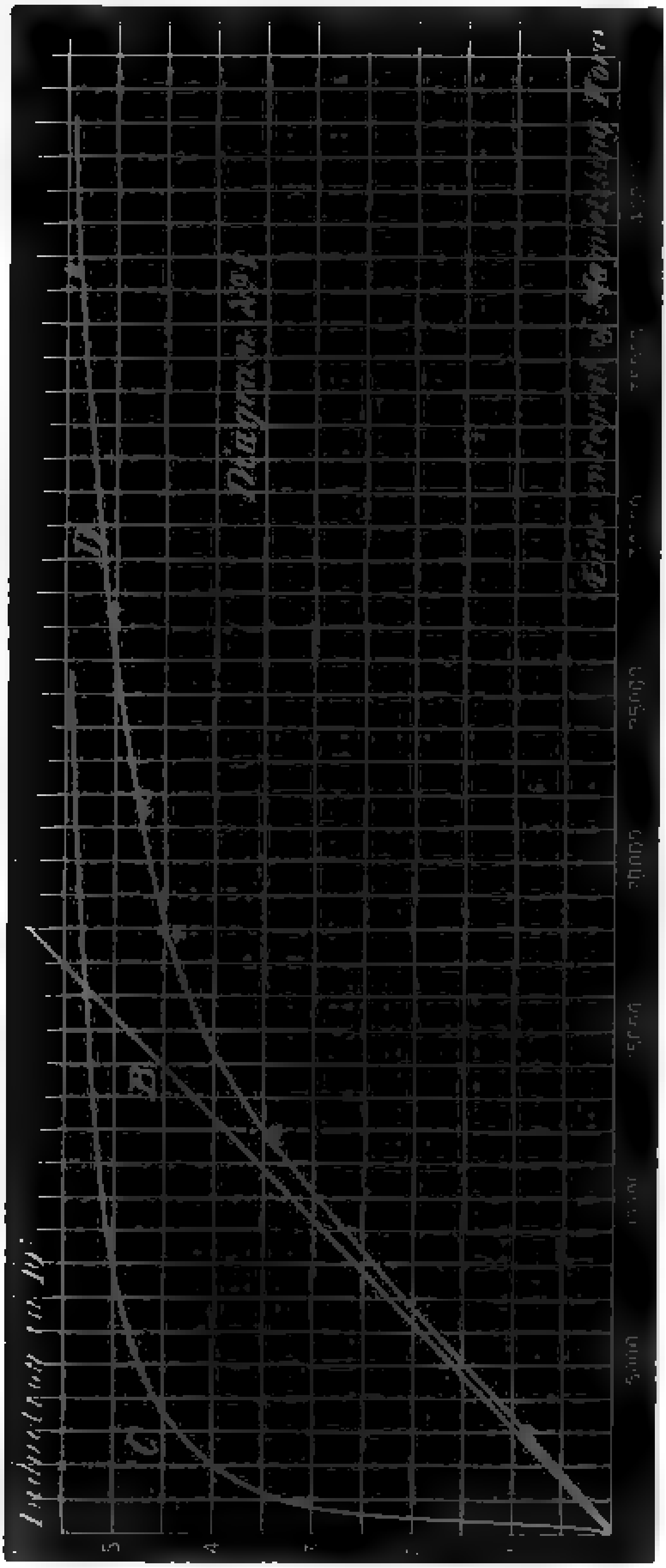
$$x = 2l_2 \frac{y}{A_2} + l_3 f\left(\frac{\nu y}{A_3}\right),$$

as given by calculation.

The marks + indicate the results of actual observations on machine No. 1, and the marks 0 the results on machine No. 2, the total induction I being given by the equation:—

$$I = \frac{\text{Potential difference in volts} \times 10^8}{208 \times \text{revolutions per second}}.$$

Experiments made upon the power taken to drive the machine under different conditions show that it takes about 250 watts more power to turn the armature at 660 revolutions when the magnets are normally excited than when they are not excited at all. The volume of the core is 9465 cub. cm., or in each complete cycle the loss per cub. centimeter is $\frac{250 \times 10^7}{11 \times 9465} = 24{,}000$ ergs.



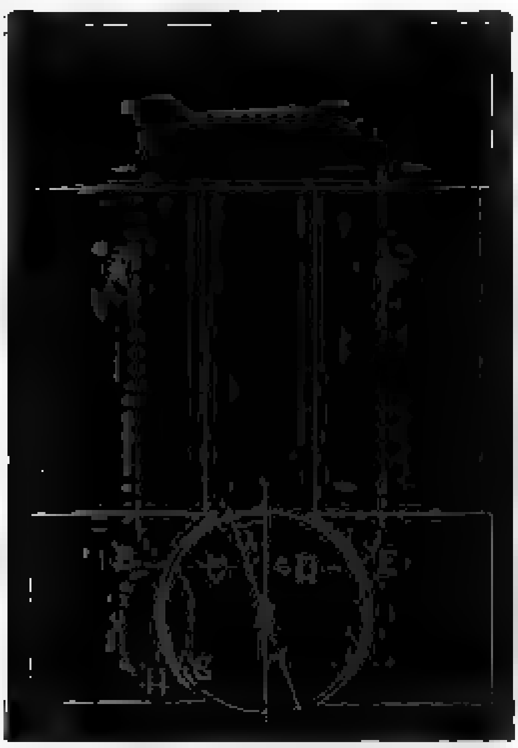
The loss by hysteresis is about 13,000 ('Phil. Trans.,' 1885, p. 463) if the reversals are made by variation of intensity of the magnetising force, and the iron is good wrought iron. This result is similar to that in the earlier paper (p. 352), where it is shown that the actual

loss in the core, when magnetised, is greater than can be accounted for by the known value of hysteresis.

Effects of the Current in the Armature.

Quoting from the Royal Society paper, p. 342, "The currents in the fixed coils around the magnets are not the only magnetising forces applied in a dynamo machine; the currents in the moving coils of the armature have also their effect on the resultant field. There are in general two independent variables in a dynamo machine, the current around the magnets and the current in the armature, and the relation of E.M.F. to currents is fully represented by a surface. In well-constructed machines the effect of the latter is reduced to a minimum, but it can be by no means neglected. When a section of the armature coils is commutated it must inevitably be momentarily short-circuited, and, if at the time of commutation the field in which the section is moving is other than feeble, a considerable current will arise in that section, accompanied by waste of power and destructive sparking.

"Suppose the commutation occurs at an angle λ in advance of the symmetrical position between the fields, and that the total current through the armature be C, reckoned positive in the direction of the resultant EMF of the machine, *i.e.*, positive when the machine is used as a generator of electricity. Taking any closed line through magnets and armature, symmetrically drawn as ABCDEFA, it is



obvious that the line integral of magnetic force is diminished by he current in the armature included between angle λ in front and

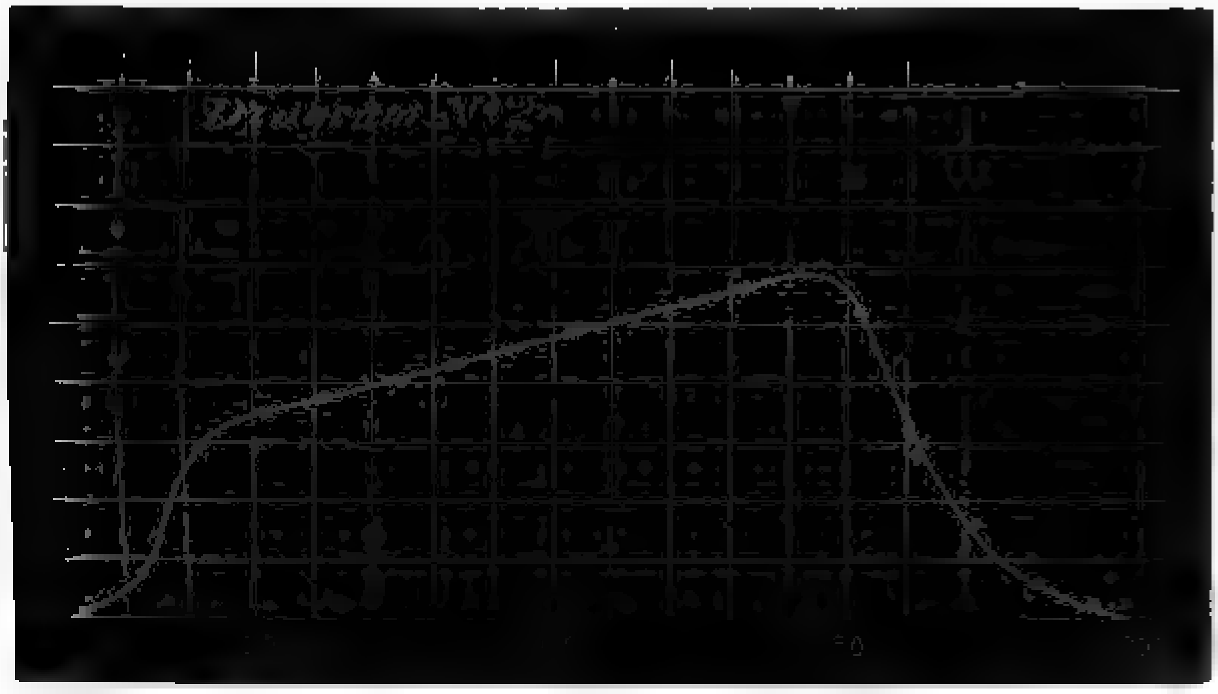
angle λ behind the plane of symmetry. If m be the number of convolutions of the armature, the value of this magnetising force is $4\pi C \frac{m}{2} \frac{2\lambda}{\pi} = 4\lambda m C$ opposed to the magnetising force of the fixed coils on the magnets. Thus, if we know the lead of the brushes and the current in the armature, we are at once in a position to calculate the effect on the electromotive force of the machine. A further effect of the current in the armature is a material disturbance of the distribution of the induction over the bored face of the pole-piece; the force along BC is by no means equal to that along DE. Draw the closed curve BCGHB, the line integral along CG, and HB is negligible. Hence the difference between force HG and BC is equal to $4\pi C \frac{m}{2} \frac{\kappa}{\pi} = 2\kappa m C$, where κ is the angle COG."

To verify this formula is one of the principal objects of this paper.

A pair of brushes having relatively fixed positions near together, and insulated from the frame and from one another, are carried upon a divided circle, and bear upon the commutator. The difference of potential between these brushes was measured in various positions round the commutator, the current in the armature, the potential difference of the main brushes, and the speed of the machine being also noted.

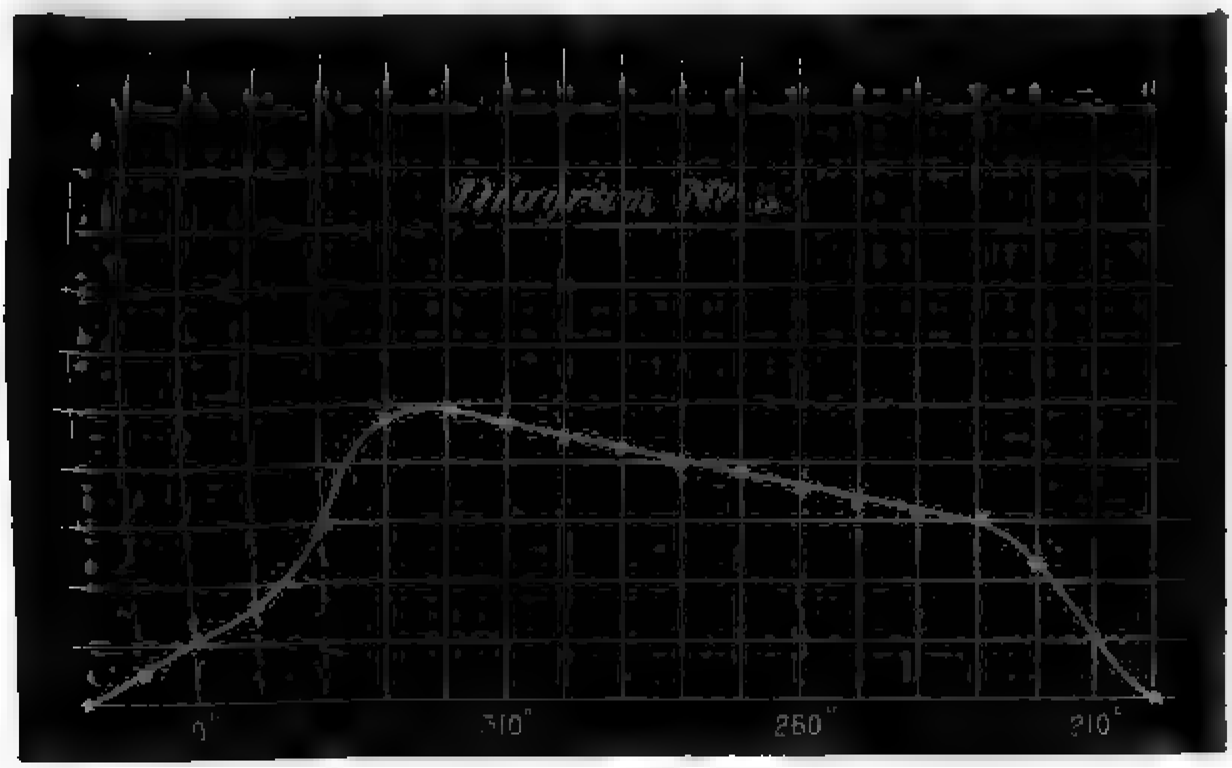
The results are given in Diagrams Nos. 2, 3, 4, and 5, in which the ordinates are measured potential differences, and the abscissæ are angles turned through by the exploring brushes. The potential differences in Diagram No. 2 were measured by a Siemens' voltmeter, and each ordinate is therefore somewhat smaller than the true value, owing to the time during which the exploring brushes were not actually in contact with the commutator segments. But this does not affect the results, because the area is reduced in the same proportion as the potential differences. In Diagrams Nos. 3, 4, and 5, the potential differences were taken on one of Sir William Thomson's quadrant electrometers, and are correct.

Take Diagram No. 2 in which machine No. 1 is a generator. A centimeter horizontally represents 10° of lead, and the ordinates represent differences of potential between the brushes. The area of the curve is 61·3 sq. cm., and represents 130 volts and a total field of $\frac{130}{104} \times \frac{1}{29} \times 10^8 = 4{\cdot}31 \times 10^6$ lines of induction. This is, of course, not the actual field, which is 3 per cent. greater on account of the resistance of the armature, but is represented by an area 3 per cent. greater. An ordinate of 1 cm. will represent an induction of $\frac{4{\cdot}31}{61{\cdot}3} \times 10^6 = 7{\cdot}0 \times 10^4$ lines in 10°. The area of 10° is $39{\cdot}5 \times 1{\cdot}73 =$



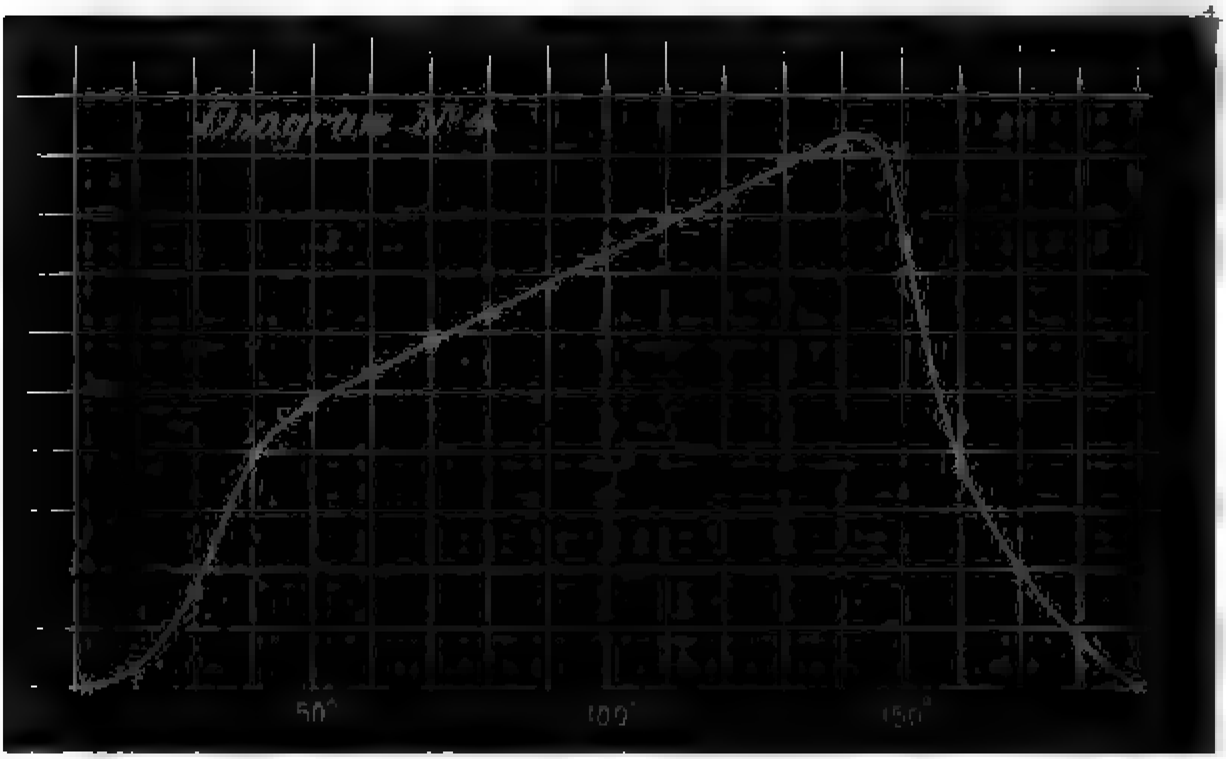
68·3 sq. cm.* Hence, an ordinate of 1 cm. represents an induction of 1024 lines per square centimeter. The difference between ordinates at 50° and 140° is 2·5; hence the difference of induction is actually 2560. Theoretically, we have $\kappa = \frac{1}{3}\pi m = 104\ C = 9{\cdot}4$. Therefore, $2\kappa m C = 3072$, and this is the line integral of magnetising force round curve.

Let A be the induction at 50° and $A+\delta$ at 140°: these also are the magnetising forces. Hence, $(A+\delta)\ 1{\cdot}4 - A\ 1{\cdot}4 = 2\kappa m C$; $\delta = 2200$ as against 2560 actually observed.



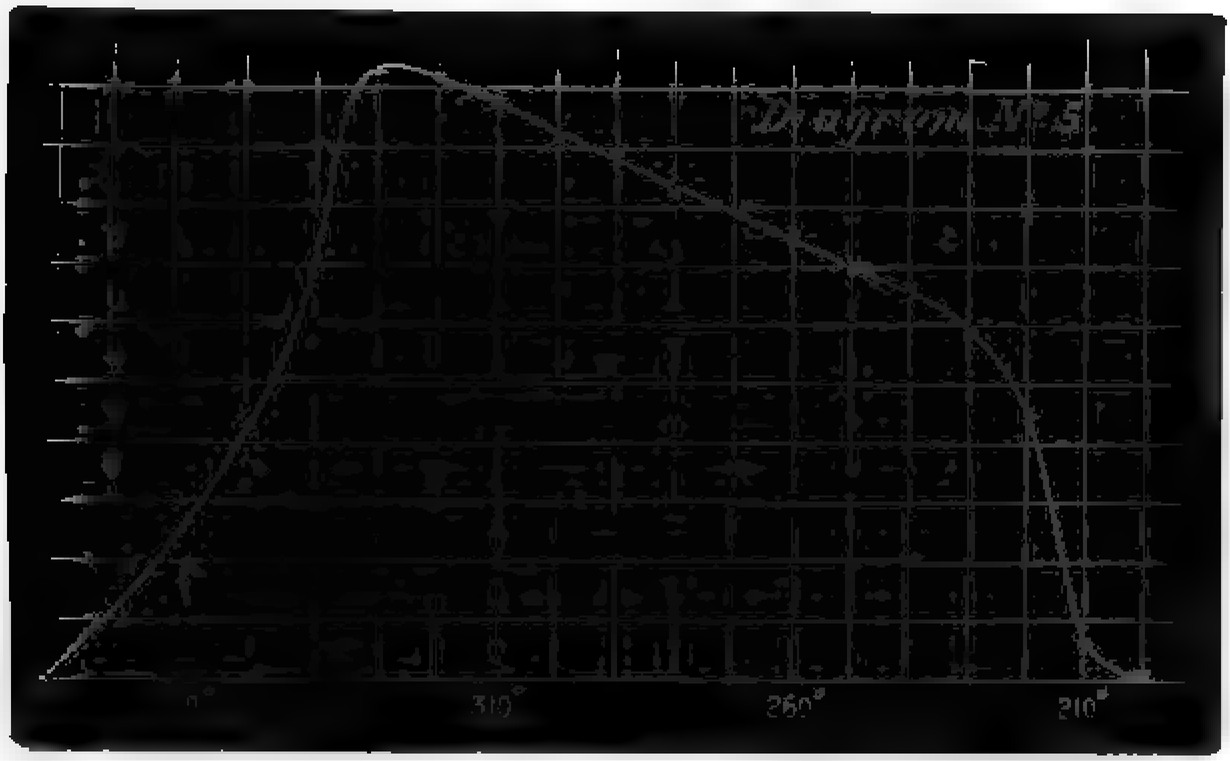
* In calculating this area, the allowance for fringe at ends of armature is taken less than before, because the form of opposing faces differs.

Take Diagram 3, in which No. 2 machine is a motor. The total field $= \frac{107}{104} \times \frac{1}{20} \times 10^6 = 5{\cdot}15 \times 10^6$ lines of induction. Since the area of the diagram is 53·5 sq. cm., an ordinate of 1 cm. $= \frac{5{\cdot}15}{53{\cdot}5} \times 10^6 =$ 96×10^4 lines of induction in 10°. Hence, an ordinate of 1 cm. represents an induction of $\frac{9{\cdot}6 \times 10^4}{68{\cdot}3} = 1400$ lines per sq. cm. The difference between ordinates at 320° and at 230° is 2·0; hence, the difference of induction is actually 2800. Theoretically, we have $\frac{2\kappa mC}{l} = \frac{3\frac{1}{4} \times 104 \times 11{\cdot}4}{1{\cdot}4} = 2666$, as against 2800 actually observed.



In Diagram No. 4, No. 1 machine is a generator. The total field $= \frac{52}{104} \times \frac{1}{12{\cdot}6} \times 10^8 = 3{\cdot}97 \times 10^6$ lines. The area of the diagram is 90·9 sq. cm., and therefore an ordinate of 1 cm. $= \frac{3{\cdot}97}{90{\cdot}9} \times 10^6 = 4{\cdot}37$ $\times 10^4$ lines in 10°. Hence, an ordinate of 1 cm. represents an induction of $\frac{4{\cdot}37 \times 10^4}{68{\cdot}3} = 639$ lines per sq. cm. The difference between ordinates at 50° and at 140° is 4·5; hence, the difference of induction is actually 2877. Theoretically, we have $\frac{2\kappa mC}{l} = \frac{3\frac{1}{4} \times 104 \times 12{\cdot}9}{1{\cdot}4} =$ 3010, as against 2877.

In Diagram No. 5, No. 2 machine is a motor. The total field $= \frac{63{\cdot}5}{104} \times \frac{1}{12{\cdot}3} \times 10^8 = 4{\cdot}96 \times 10^6$ lines. The area of the diagram is



112·2 sq. cm., and therefore an ordinate of 1 cm. $= \frac{4·96}{112·2} \times 10^6 =$ $4·42 \times 10^4$ lines in 10^6. Hence, an ordinate of 1 cm. represents an induction of $\frac{4·42}{68·3} \times 10^6 = 647$ lines per sq. cm. The difference between ordinates at 323° and at 233° is 4·2; hence, the difference of induction is actually 2718. Theoretically, we have $\frac{2\kappa mC}{l} = \frac{3\frac{1}{4} \times 104 \times 12·3}{1·4} =$ 2870, as against 2718 actually observed.

At page 345 of the paper on Dynamo-Electric Machinery it is shown that

$$\mathrm{I} + \frac{\nu - 1}{\nu}\, 4\,\lambda m C\, \frac{A_2}{2l_2} = \mathrm{F}\left(4\,\pi n c - \frac{4\,\lambda m C}{\nu}\right),$$

where $\mathrm{I} = \mathrm{F}(4\pi n c)$ is the characteristic curve when $C = 0$, and λ is the lead of the brushes.

The following is an endeavour to verify this formula. The potentials both upon the magnets and upon the brushes were taken by a Siemens' voltmeter, and are rough. The speeds were taken by a Buss tachometer, and there is some uncertainty about the precise lead of the brushes, owing to the difficulty in determining the precise position of the symmetrical position between the fields, and also to the width of the contacts on the commutator.

It was necessary, in order to obtain a marked effect of the armature reaction, that the magnet field should be comparatively small, that the current in the armature should be large, and the leads of the brushes should be large.

The two machines had their axles coupled so that No. 1 could be

run as a generator, and No. 2 as a motor. The magnets were in each case coupled parallel, and excited by a battery each through an adjustable resistance. The two armatures were coupled in series with another battery and the following observations were made :—

	Potential on magnets in volts.	Potential on brushes.	Speed per minute.	Current in ampères.	Lead of brushes.
No. 1	24—24	66—67	880	102—103	26°
No. 2	29—29	86—84	880	102—103	29°

From which we infer :—

	Current in magnets.	$4\pi nc$.	Corrected potential for resistance of armature.	Total induction I.
No. 1	1·78	8,900	70·8	$2·30 \times 10^6$
No. 2	2·15	10,750	80·7	$2·65 \times 10^6$

As there was uncertainty as to the precise accuracy of the measurements of potential, it appeared best to remeasure the potentials with no current through the armature with the Siemens' voltmeter placed as in the last experiment. Each machine was theretore run on open circuit with its magnets excited, and its potential was measured.

	Potential on magnets in volts.	Potential on brushes.	Speed per minute.	Potential at 880 revs.
No. 1	25—25	90—90	880	90·0
No. 2	28—28	79—80	715—710	98·2

From which, since the formula is reduced to

$$I = \frac{A_2}{2l_2}(4\pi nc - 4\lambda m C),$$

the characteristic being practically straight, we infer :—

	Potential on magnets.	Potential on brushes.	Induction $I = F(4\pi nc)$.
No 1	24	86·4	$2{\cdot}82 \times 10^6$
No. 2	29	101·7	$3{\cdot}30 \times 10^6$

We have further:—

$\lambda = 0{\cdot}45$ for No. 1. $\qquad \lambda = 0{\cdot}5$ for No. 2

$$\frac{4mC}{\nu} = 2920 \qquad \frac{\nu-1}{\nu}4mC\frac{A_2}{2l_2} = 443800.$$

	$\frac{4\lambda mC}{\nu}$.	$\frac{\nu-1}{\nu}4\lambda mC\frac{A_2}{2l_2}$.	$4\pi nc - \frac{4\lambda mC}{\nu}$.	$F(4\pi nc - \frac{4\lambda mC}{\nu})$.	$F(4\pi nc - \frac{4\lambda mC}{\nu}) - \frac{\nu-1}{\nu}4\lambda mC\frac{A_2}{2l_2}$.
No. 1	1314	199700	7586	$2{,}41 \times 10^6$	$2{\cdot}21 \times 10^6$
No. 2	1460	221900	9290	$2{,}90 \times 10^6$	$2{\cdot}68 \times 10^6$

It has already appeared that experiment gives for I in No. 1 $2 \cdot 3 \times 10^6$, and in No. 2 $2 \cdot 65 \times 10^6$. The difference is probably due to error in estimating the lead of the brushes, which is difficult, owing to uncertainty in the position of the neutral line on open circuit.

II. "On the Clark Cell as a Standard of Electromotive Force." By R. T. GLAZEBROOK, M.A., F.R.S., Fellow of Trinity College, and S. SKINNER, M.A., Christ's College, Demonstrator in the Cavendish Laboratory, Cambridge. Received February 17, 1892.

(Abstract.)

The paper consists of two parts:—

In Part I an account is given of experiments on the absolute electromotive force of a Clark cell.

This was determined in the manner described by Lord Rayleigh ('Phil. Trans.,' 1884) in terms of a known resistance and the electrochemical equivalent of silver.

The resistance used was a strip of platinoid about 1 cm. wide and 0·05 cm. thick wound on an open frame. It was immersed in a bath of paraffin oil, and the currents used, varying from about 0·75 to rather over 1·4 ampères, did not raise its temperature sufficiently to affect the result. It had a resistance of nearly 1 B.A. unit. This was determined in terms of the original B.A. units. As part of the object of the experiments was to test the memorandum on the use of the silver voltameter recently issued by the Electrical Standards Committee of the Board of Trade, the large currents mentioned above were purposely employed. The silver voltameters were treated in accordance with the instructions in the memorandum.

The standard cell to which the results are referred is one constructed by Lord Rayleigh in 1883, probably No. 4 of the cells described in his paper already quoted.

The results have been reduced on the supposition that 1 B.A. unit is equal to 0·9866 ohm; if we take the number 0·9535* as representing the value in B.A. units of the resistance of a column of mercury at 0°, 1 metre long, 1 sq. mm. in section, the above is equivalent to saying that the length of the mercury column having a resistance of 1 ohm is 106·3 cm. It has also been assumed that the mass of silver deposited in one second by a current of 1 ampère is 0·001118 gramme, and that the coefficient of change of E.M.F. with temperature of a Clark's cell is 0·00076. This last result has been verified by us in Part II.

* This number is the mean of the best recent results.

An account of nine separate experiments is given in the paper; the following are the results reduced to 15° C.:—

No. of experiment.	E.M.F. of cell.	No. of experiment.	E.M.F. of cell.
1	1·4341	6	1·4342
2	1·4336	7	1·4342
3	1·4341	8	1·4340
4	1·4340	9	1·4345
5	1·4340		

The mean of these is 1·4341, or, correcting for the rate of the clock, 1·4342.

In Experiment 2 the current in the voltameter was rather unsteady, which may account for the low value; while in Experiment 9 the temperature of the cell was changing somewhat, and our later experience has shown us that the E.M.F. in our standard cell lags very considerably behind the temperature. Still even taking these experiments into account, the results are very close.

If we suppose, as seems most probable, for reasons given in the paper, that our cell is No. 4 of Lord Rayleigh's paper, and that it has retained relative to No. 1 (Lord Rayleigh's standard) the value it had in 1883, the E.M.F. of his cell No. 1 would be in the units he used

1·4346 volts at 15°.

The value found by Lord Rayleigh was 1·4348 volts; thus the two are very close.

In the units we have given above, those specified by the Board of Trade, we have finally the result that the E.M.F. of our cell is

1·4342 volts at 15° C.
or 1·4324 volts at 62° F.

Part II.

In the second part of the paper we have investigated some of the sources of error in the Clark cell, and also the effects of small variations in the materials used and the method of their preparation. We have also compared a number of cells set up by different makers. The general result is a very good agreement among cells from very various sources.

Cells set up by Lord Rayleigh in 1883 and 1884, Mr. Elder in 1886, Mr. H. L. Callendar in 1886, Dr. Muirhead in 1890, and by Dr.

Schuster, Mr. Wilberforce, and ourselves during the past year, all agree closely, the variations among them being rarely greater than about 0·0005 volt.

The first set of cells, eighteen in number, constructed for the purposes of this enquiry were made according to Lord Rayleigh's instructions, using, however, various specimens of the chemicals. These showed some differences at first, but in the course of about two months they had all, with one exception, settled down to close agreement with the standard. The exceptional cell has since become normal. In two of these cells mercury was used which had been taken direct from the stock in every-day use in the laboratory. The E.M.F. of these cells was much too low at first, but it gradually increased, and they are now normal. The mercurous sulphate appears to free the mercury from certain harmful impurities.

Another set of cells were put up, in accordance with the provisional memorandum of the Electrical Standards Committee of the Board of Trade, issued in June last and quoted below.

Memorandum on the Preparation of the Clark's Standard Cell.

Definition of the Cell.

The cell consists of mercury and zinc in a saturated solution of zinc sulphate and mercurous sulphate in water, prepared with mercurous sulphate in excess, and is conveniently contained in a cylindrical glass vessel.

Preparation of the Materials.

1. *The Mercury.*—To secure purity it should be first treated with acid in the usual manner, and subsequently distilled in vacuo.

2. *The Zinc.*—Take a portion of a rod of pure zinc, solder to one end a piece of copper wire, clean the whole with glass paper, carefully removing any loose pieces of the zinc. Just before making up the cell, dip the zinc into dilute sulphuric acid, wash with distilled water, and dry with a clean cloth or filter paper.

3. *The Zinc Sulphate Solution.*—Prepare a saturated solution of pure ("pure recrystallised") zinc sulphate by mixing in a flask distilled water with nearly twice its weight of crystals of pure zinc sulphate, and adding a little zinc carbonate to neutralise any free acid. The whole of the crystals should be dissolved with the aid of gentle heat, *i.e.*, not exceeding a temperature of 30° C., and the solution filtered, while still warm, into a stock bottle. Crystals should form as it cools.

4. *The Mercurous Sulphate.*—Take mercurous sulphate, purchased as pure, and wash it thoroughly with cold distilled water by agitation in a bottle; drain off the water, and repeat the process at least twice. After the last washing, drain off as much of the water as possible.

Mix the washed mercurous sulphate with the zinc sulphate solution, adding sufficient crystals of zinc sulphate from the stock bottle to ensure saturation, and a small quantity of pure mercury. Shake these up well together to form a paste of the consistency of cream. Heat the paste sufficiently to dissolve the crystals, but not above a temperature of 30°. Keep the paste for an hour at this temperature, agitating it from time to time, then allow it to cool. Crystals of zinc sulphate

should then be distinctly visible throughout the mass; if this is not the case, add more crystals from the stock bottle, and repeat the process.

This method ensures the formation of a saturated solution of zinc and mercurous sulphates in water.

The presence of the free mercury throughout the paste preserves the basicity of the salt, and is of the utmost importance.

Contact is made with the mercury by means of a platinum wire about No. 22 gauge.—This is protected from contact with the other materials of the cell by being sealed into a glass tube. The ends of the wire project from the ends of the tube; one end forms the terminal, the other end and a portion of the glass tube dip into the mercury.

To set up the Cell.

The cell may conveniently be set up in a small test-tube of about 2 cm. diameter, and 6 or 7 cm. deep. Place the mercury in the bottom of this tube, filling it to a depth of, say, 1·5 cm. Cut a cork about 0·5 cm. thick to fit the tube; at one side of the cork bore a hole, through which the zinc rod can pass tightly; at the other side bore another hole for the glass tube which covers the platinum wire; at the edge of the cork cut a nick through which the air can pass when the cork is pushed into the tube. Pass the zinc rod about 1 cm. through the cork.

Clean the glass tube and platinum wire carefully, then heat the exposed end of the platinum red hot, and insert it in the mercury in the test-tube, taking care that the whole of the exposed platinum is covered.

Shake up the paste and introduce it without contact with the upper part of the walls of the test-tube, filling the tube above the mercury to a depth of rather more than 2 cm.

Then insert the cork and zinc rod, passing the glass tube through the hole prepared for it. Push the cork gently down until its lower surface is nearly in contact with the liquid. The air will thus be nearly all expelled, and the cell should be left in this condition for at least twenty-four hours before sealing, which should be done as follows:—

Melt some marine glue until it is fluid enough to pour by its own weight, and pour it into the test-tube above the cork, using sufficient to cover completely the zinc and soldering. The glass tube should project above the top of the marine glue.

The cell thus set up may be mounted in any desirable manner. It is convenient to arrange the mounting so that the cell may be immersed in a water-bath up to the level of, say, the upper surface of the cork. Its temperature can then be determined more accurately than is possible when the cell is in air.

These cells, as the tests given show, have been good from the first, and, indeed, we have not had any difficulty with any of the cells in which the instructions of this memorandum have been followed.

The mercury used had been distilled in the laboratory, the zincs were supplied as "pure" by Messrs. Harringtons, of Cork, while the zinc and mercurous sulphates came from Messrs. Hopkin and Williams.

The numbers in the table show the differences between the cells and the standard; the unit is 0·00025 volt.

Differences between a Set of Cells and the Standard.

Date........	June 4.	June 6.	June 9.	July 20.	Aug. 6.	Aug. 10.	Aug. 14.	Aug. 22.	Nov. 2.	Nov. 14.	Dec. 17.	July 7.*	July 18.*
Temperature..	16	16	14·5	18	16·2	16·4	17·5	16·4	14·4	9·2	15	15	15
No. 71	−4	−1	−1	0	0	0	0	0	−1	−2	−2	2	−4
,, 72	−3	−1	−1	1	1	0	0	0	1	1	1	3	2
,, 73	−8	−8	−8	1	1	0	1	0	1	1	0	1	2
,, 74	−2	1	2	2	1	0	0	0	1	1	−1	4	2
,, 75	−5	−1	0	0	1	0	1	−1	1	1	−1	4	2
,, 76	−3	2	−1	0	−1	0	−2	−1	1	0	0	2	3

* Comparison with the standard of the Board of Trade. The unit is 0·00025 volt.

It may be well to explain the purpose of some of the precautions advised in the circular. The mercurous sulphate, as ordinarily purchased, contains some mercuric sulphate. When this is moistened with water it is resolved into a yellow basic mercuric sulphate (turpeth mineral) and a soluble acid mercuric sulphate. The first, at any rate in moderate quantities, does not affect the E.M.F.; the latter greatly hinders it from attaining the proper value. Repeated washing, however, removes most of this soluble salt. The paste, when made, is shaken with mercury to remove any traces of the acid sulphate which may be left, for the acid mercuric sulphate attacks the mercury and forms mercurous sulphate.

Careful precautions are necessary to ensure that the solutions should be saturated with both zinc and mercurous sulphates, but the solutions should not be raised in temperature above 30° C., for the zinc sulphate may then crystallise out in the wrong form. The proper crystals have the composition $ZnSO_4.7H_2O$, and are rhombic.

But while we have had no serious difficulty with any of the cells prepared in accordance with the last form of the memorandum, some of the other cells we have set up have led to some interesting results.

Two sets of cells were put up with great care by Mr. Wilberforce in March and April. One of us (S. S.) set up some cells in the same way about the same time. The solutions were prepared from very pure materials, following Lord Rayleigh's instructions. The zinc sulphate was remarkably free from acid, and it appeared as if the results ought to be good.

In the first set, Nos. 36—41, the E.M.F. was too low. At the end of a month it was much too low, about 0·005 volt, and Mr. Wilberforce noticed that a dull-grey deposit covered the zincs; he therefore removed them and scraped off this deposit, when, on replacing the zincs, the cells were found to have approximately the normal E.M.F.; they have continued nearly normal since. The next set, Nos. 42—47, were very good when first set up, but the E.M.F. soon fell rapidly, until at the end of a month they were nearly 0·01 volt too low.

The grey deposit again was formed over the zinc. Some of these cells were left untouched till August, by which time the E.M.F. had recovered somewhat, being then about 0·005 too low. Others had been treated by removing the zincs and replacing them by amalgamated zincs. In August some experiments were made on the unaltered cells, which showed conclusively that it is necessary that the surface of the zinc should remain bright if consistent results are to be obtained.

This bright surface may be secured by amalgamating the zinc, but we are not yet sure that this alone is effective, for it seems possible from various observations that some action which results in the

amalgamation of the zinc must go on in the cell to enable it to reach the steady state, and that it may not be sufficient to introduce amalgamated zincs. On this and some kindred points, however, we are still experimenting. The grey deposit can be shown to be mainly mercury in a state of very fine division. There are some indications that a slight acidity in the solutions is of use in promoting amalgamation.

We have verified repeatedly an observation of Dr. Hopkinson's that the E.M.F. of a bad cell changes considerably if the cell be slightly shaken, while that of a good cell is not affected.

The paper also contains an account of some experiments on the coefficient of change of E.M.F. with temperature. The value found is 0·000755 per 1° C., practically the same as that given by Lord Rayleigh. In this connexion we may mention the important observation that when the temperature is rising, even although the rise be only a few degrees, the E.M.F. of the cell may—especially if the cell be large—lag very considerably behind the temperature. On one occasion in which the temperature rose by some 5° C. in about a week, the E.M.F. of our large cell at the end of the week corresponded to a temperature nearly 3° lower than that given by a thermometer in the bath with the cell, being about 0·0027 volt too high. In this case a thick cake of crystals had formed on the top of the more solid portion of the paste, and the zinc sulphate solution only attained the state of saturation corresponding to the temperature by very slow degrees. Mr. Carhart and Mr. Swinburne have called attention to the difficulties which thus attend the practical use of the cells. They are to some extent met by using small cells.

The paper also describes a new form of portable cell which may be turned into any position without harm. Experiments have also been made on the mercury chloride standards described by Von Helmholtz. A set of these has been constructed which has an E.M.F. of very nearly 1 volt. A form of standard due to Gouy, in which oxide of mercury is used, has also been examined. The E.M.F. of these cells prepared with yellow oxide is, we find, 1·381 volts, and when prepared with red oxide 1·388 volts.

By the kindness of Major Cardew several of our cells have been compared with the standards of the Board of Trade. The differences are very small, being about 0·0003 volt. The average of the Board of Trade cells is less than our standard by about this amount.

The Board of Trade possess seventy-two cells, and Mr. Rennie, Major Cardew's Assistant, informs us that the greatest difference between any two of them is under 0·0007 volt. It will be seen from the table given that, while the cells there considered are on the average about one of our units above our standard, they are rather over two of such units above the Board of Trade cells.

Thus our standard exceeds the cells of the Board of Trade by rather over one of our units, or about 0·0003 volt.

If we take the E.M.F. of our standard as 1·4342 volts at 15°, the cells of the Board of Trade average in E.M.F. about 1·4339 volts at 15° C., or 1·4321 volts at 62° Fahr.

III. "Note on the Functional and Structural Arrangement of Efferent Fibres in the Nerve-roots of the Lumbo-sacral Plexus." (Preliminary Communication.) By C. S. Sherrington, M.A., M.B., &c. Communicated by Professor M. Foster, Sec. R.S. Received March 14, 1892.

At the commencement of some observations on the reflex mechanisms of the spinal cord in *Macacus rhesus*, difficulties were encountered which made it desirable to attempt for that animal a somewhat particular examination of the distribution of the efferent and afferent spinal nerve-roots belonging to the lumbo-sacral plexus. The present communication has reference to the distribution of the efferent fibres of the roots.

Reil,* Scarpa,† A. Monro,‡ and Soemmering§ all paid considerable attention to the arrangement of the root-bundles in the limb plexuses, but physiological work upon the subject commenced with Van Deen,‖ J. Müller,¶ and Panizza.** The former two gave an anatomical significance to the plexus, the last a physiological. At Müller's suggestion, renewed research was undertaken by H. Kronenberg†† in 1835. Kronenberg confirmed Müller's observations as to the individual inconstancy of the contribution made by any spinal root to the nerve cords of the plexus; he also concluded that the excitation of a single nerve-root before its entrance into the plexus produces contraction of almost all the muscles of the limb; and that the arrangement is intended to protect against fatigue. Later, Eckhardt,‡‡ working in Ludwig's laboratory, arrived at somewhat similar conclusions. He stated that a great number of muscles obtain nerve-fibres each of them from several nerve-roots; that there is a good deal of individual variation; that when a nerve-root is

* 'De Nervorum Structura,' p. 14.

† 'De Gangliis et Plexibus.'

‡ 'Observations on the Structure and Functions of the Nervous System,' p. 34.

§ 'Anatom.,' Pars Vta.

‖ 'De Differentia et Nexu inter Nervos Vitæ Anim. et Organ.,' Leyden, 1834.

¶ 'Physiol. des Menschen,' vol. 2, p. 586.

** 'Annali Universali di Medicina.'

†† Essay ('De Struct. et Virtut. Plexuum Nervorum'), Berlin, 1836.

‡‡ 'Zeits. f. Rat. Med.,' vol. 7, p. 306, 1849.

unusually thick the additional fibres in it are not all of them, perhaps none of them, used to supply the muscles usually supplied by the root, but are used to supply altogether other muscles not usually supplied by the roots; that the distribution of the fibres of a root is not to one group of muscles, but is to several groups, which are often not related to each other in function; that antagonistic groups are often supplied by one and the same root.

Three years after the experiments by Eckhardt, and also under Ludwig, Peyer's* experiments on the brachial plexus of the rabbit were made. As Krause, in 1861,† repeated Peyer's work on the same limb and the same species, the results of both may be here referred to together. The muscles of the limb each receive nerve-fibres from two, in some cases three, spinal roots; usually the contraction of a muscle on excitation of the spinal roots innervating it is obviously different in degree for each root: the same spinal root does not always supply in different individuals the same muscles; the further the position of a spinal root from the head, the nearer the muscles it supplies to the distal end of the limb; the peripheral trunks of the limb plexus are themselves plexuses of root-bundles. In 1881 Ferrier and Yeo‡ confirmed the above results in experiments on the spinal roots of the monkey. In addition to their experiments on the brachial plexus, they performed four complete experiments on the lumbo-sacral roots. Unlike Kronenberg, Eckhardt, and others, they do not seem to have met with any variation in the results obtained. They revived the view that the efferent distribution of each spinal nerve is based on its physiological function, and that the movement resulting from the excitation of a root is that of a highly coordinated functional synergism. Some months later Paul Bert and Marcacci§ published experiments on the lumbar roots of the cat and dog. They concluded that (i) each root produces a coordinate movement, and consists of fibres functionally associated; (ii) when a muscle is functionally divisible its root-supply is multiple.

In 1883 Forgue and Lannegrace‖ published a research on the limb plexuses of the cat, dog, and monkey. The 'Comptes Rendus'¶ of the following year contain their reports. As to the lower limb, their account is prefaced by a remark that the highest lumbar root of man is tripled in the dog and monkey. What species of monkey was used is not mentioned in the 'Comptes Rendus.' In *Macacus* the 5th lumbar root is analogous not to the 3rd of man, but to the 4th, and

* 'Arch. f. Rat. Med.,' II, vol. 4, p. 67, 1853.

† 'Beiträge zur Anat. der Oberen Extremität,' 1861.

‡ 'Roy. Soc. Proc.,' 1881.

§ 'Soc. de Biol.,' July, 1881.

‖ 'Gaz. Hebd. des Sci. Médic. de Montpellier,' 1883.

¶ 'Comptes Rendus,' 1884, vol. 98, pp. 829, 685, 1068.

to the 6th of the dog. The chief of their conclusions, drawn from examination of both limbs, are:—The majority of muscles are innervated by several roots. Excitation of a root determines in the muscles which it supplies a total, not a partial, contraction. The tributary fibres of the root are disseminated through the muscle supplied by it, and not "cantonnées" in a special zone of it. Each root has a muscular distribution almost absolutely constant in the animals of its own species. The functions of analogous roots differ very little in different mammalian species. Each root supplies muscles of very various, often of antagonistic, action. Excitation of a root gives a combined movement, but an artificial, not a functional. The roots that pass furthest into the member occupy the lowest position in the cord. The innervation of the two planes of flexors and extensors is not always symmetrical. The superficial layers are supplied before the deep.

Herringham,* by minute dissection of the human brachial plexus, and, therefore, under disadvantage from inability to distinguish clearly between afferent and efferent fibres, arrived nevertheless at facts and conclusions of great importance. He found much individual variation, but evidence of certain "laws." Thus: any given root-fibre may alter its position relative to the vertebral column, but will maintain its position relative to other fibres; of two muscles, or two parts of a muscle, that which is nearer the head end of the body tends to be supplied by the higher, that nearer the tail end by the lower, root; of two muscles, that nearer the long axis of the body tends to be supplied by the higher, that nearer the periphery by the lower, root; of two muscles, that nearer the surface tends to be supplied by the higher, that further from it by the lower, root.

Recently Langley,† in the course of a paper on the sweat nerves to the foot of the cat, refers to the movements of the limb produced by excitation of roots of sciatic plexus in that animal. He desired to ascertain whether the variation, which he finds considerable in the distribution of the sweat nerves (sympathetic system), has a correlative in the distribution of the nerves to the limb muscles. Like Kronenberg, Eckhardt, and Peyer, he finds that the movements resulting from stimulation of the same nerve-roots are not uniform, and that the want of uniformity goes hand in hand with want of uniformity in the root composition of the plexus, just such as displayed in Herringham's dissections.

My own observations have been made, during the past three years, chiefly on the lumbo-sacral roots of *Macacus rhesus;* also on the frog, rat, rabbit, cat, and dog, chiefly for the sake of comparing those types with *Macacus.* The animals have been deeply anæsthetised

* 'Roy. Soc. Proc.,' 1887.

† 'Jl. of Physiol.,' September, 1891.

with chloroform and ether. The excitations of the roots have been made in the spinal canal; the single root, or a component filament from it, has been isolated in the case of the lower roots of the cat and monkey, to a length 5, 6, or 7 cm., and lifted up by a silk ligature on to small platinum electrodes sheathed almost to the points. Series of weak induced currents have been used for excitation, one pint Daniell being in the primary of the ordinary physiological inductorium (R. Ewald's pattern). The secondary coil has usually been at a distance from the primary somewhat more than twice that at which a current was detectable by the tongue. Use has also been made of absolutely minimal stimuli, and largely of mechanical stimuli. For certain purposes, stimulation by quite strong electrical excitation has been used.

In these experiments it became clear that the frequency of individual variation, as regards the anatomical and physiological constitution of the efferent roots of the lumbo-sacral region, was great enough to demand the recognition of a "pre-axial" and a "post-axial" class of innervation for each muscle and movement. By pre-axial class of innervation is meant that the roots connected with the muscles and the movements are more pre-axial than the roots connected with the same muscles and the same movements in the post-axial class of innervation.

Thus, in the frog there is a pre-axial class of innervation for the hind limb, in which, for instance, the viith spinal root, as well as supplying the antero-internal thigh muscles, supplies the muscles on the front of the leg (tibialis anticus). There is a post-axial class in which the pretibial muscles are supplied by the viiith and ixth roots only. The post-axial class as measured in this way is the more usual. This may be merely because the above criterion, found convenient for distinguishing in any individual case the direction which the variation has taken in it, does not coincide with the mid point about which individual variation in the species is really oscillating.

By "pre-axial" and "post-axial" classes it is not intended to imply that in the range of individual variation one case is not more frequently exemplified than are others; it is only meant that so frequent is the variation that no one case is sufficiently predominant to warrant the choice of it as a "normal" type, and that therefore it is more correct to treat the composition of the plexus of the species as multiple and then for convenience divide it into classes. I have thought two classes, "pre-axial" and "post-axial," a distinction sufficient to observe in my present description. Just as in the frog, so in the other animals employed, the "pre-axial" and "post-axial" class of the plexus have both been exemplified by individuals of each species. In the rat, rabbit, cat, and dog, the 9th subthoracic root sometimes supplies the intrinsic muscles of the foot (post-axial innervation), some-

times does not, the 8th taking its place (pre-axial class of innervation) as well as supplying other fibres also. In the cat, in my own experiments, the post-axial class, as measured by the above standard, has contained a rather larger number of individuals (twenty-two out of thirty-nine).*

In experiments on *Macacus*, also, it early became clear that the types of innervation of the limb-muscles by the spinal roots are conveniently dealt with as two classes. In fifty-two individuals the reactions obtained place the majority (thirty-one) within a pre-axial class, the broad features of which are as follows:—

Pre-axial Class of Innervation.

No. of the root excited.	Movement produced.
1st Subthoracic ..	Retraction of abdominal wall, near umbilicus in front.
2nd " ..	Retraction of lower part of abdominal wall, flexion of hip, with some eversion, drawing up of testicle (stronger than with 3rd).
3rd " ..	Retraction of lowest part of abdominal wall, drawing up of the testicle, flexion at hip, with marked rotation of thigh outwards and some adduction, some extension of knee.
4th " ..	Flexion at hip, extension at knee, adduction of thigh, eversion of thigh.
5th " ..	Flexion at hip, extension at knee, adduction of thigh, strong flexion at ankle, drawing up of outer edge of foot, flexion of hallux and digits.
6th " ..	Extension at hip, adduction of thigh, flexion at knee, extension at ankle, rotation of leg inwards, lifting of outer edge of foot, flexion of digits and hallux at terminal joint with (sometimes) adduction.
7th " ..	Extension at hip, with slight rotation outward of the thigh, flexion at knee, extension at ankle, flexion of digits and hallux with adduction of hallux, depression and adduction of root of tail, closure of anus.
8th " ..	Extension at hip, with slight rotation outward of the thigh, flexion at knee, extension at ankle, strong flexion and adduction of hallux, flexion of digits in "interosseal" position, closure and protrusion of anus, root of tail adducted and depressed, perineum pushed down.

* This agrees with the observations by Langley, *loc. cit.*

No. of the root excited.	Movement produced.
9th Subthoracic ..	Adduction of root of tail, which is drawn toward the side stimulated.
10th	Proximal half of tail drawn toward side stimulated.

No. of root excited.	Muscle thrown into action.
1st Subthoracic ..	Quadratus lumborum, psoas parvus, external oblique, internal oblique, transversalis.
2nd „ ..	Quadratus lumborum, psoas magnus, cremaster, external oblique, internal oblique, transversalis.
3rd „ ..	Psoas, cremaster, iliacus, external oblique, internal oblique (lower part only), transversalis, pectineus, adductor longus, sartorius (upper part especially), vastus internus, and obdurator externus slightly.
4th „ ..	Psoas, iliacus, pectineus, adductor longus, sartorius (lower part especially), vastus internus (> vastus externus), crureus, obturator, rectus femoris, vastus externus, gracilis.
5th „ ..	Gracilis, vastus externus (> vastus internus), rectus femoris, vastus internus, crureus, adductor magnus, semimembranosus, tibialis anticus, tensor vaginæ femoris, peroneus longus (occasionally strongly), flexor longus hallucis (slight), flexor longus digitorum (slight), tibialis posticus (slight), extensor longus digitorum, extensor proprius hallucis.
6th „ ..	Tibialis anticus, extensor longus digitorum, extensor hallucis, peroneus longus, peroneus brevis, extensor brevis digitorum, gastrocnemius external head (> internal head), internal head, tibialis posticus, flexor longus digitorum, flexor longus hallucis, semimembranosus (> semitendinosus), semitendinosus, biceps (slight, chiefly in deep portion), adductor hallucis, flexor brevis digitorum, adductor hallucis (slight), adductor minimi digiti, soleus (slight), plantaris, popliteus, gluteus medius, quadratus femoris.
7th „ ..	Tibialis anticus, extensor longus digitorum, extensor proprius hallucis, peroneus longus,

No. of root excited.	Muscle thrown into action.
	slight (< peroneus brevis), peroneus brevis, gastrocnemius external head, internal head, plantaris, tibialis posticus, flexor longus digitorum, soleus, flexor longus hallucis, extensor brevis digitorum, flexor brevis digitorum, adductor hallucis, adductor minimi digiti, flexor accessorius, flexor brevis hallucis, flexor brevis minimi digiti, interossei and lumbricales, obturator internus, quadratus femoris, gemelli superior et inferior, pyriformis (the larger part of, especially the lateral part), deeper part of sphincter ani, semitendinosus, semimembranosus, biceps, adductor magnus (part of), popliteus, gluteus medius.
8th Subthoracic ..	Biceps (< than semimembranosus), semitendinosus (< than semimembranosus), semimembranosus, gluteus maximus, gluteus medius, gastrocnemius internal head (< than external), external head, soleus, adductor hallucis, flexor accessorius, adductor minimi digiti, adductor hallucis, obturator internus, quadratus femoris (slight), gemelli superior et inferior, pyriformis (small part, chiefly mesial), sphincter ani, flexor brevis hallucis, flexor brevis minimi digiti, lumbricales and interossei.
9th „ ..	Sphincter vaginæ, obturator internus (slightly).

The remaining twenty-one individuals formed a "post-axial" class, with the following broad characters in common :—

Root.	Movement.
1st Subthoracic .. (1st lumbar)	Retraction of abdominal wall.
2nd „ ..	Retraction of lower part of abdominal wall, drawing up of testicle, slight flexion at hip.
3rd „ ..	Contraction of lower part of abdominal wall, drawing up of testicle (stronger than with 2nd), flexion at hip, slight flexion at knee, slight rotation outwards of thigh.
4th „ ..	Drawing up of testicle (slight) ?, contraction of lower part of abdominal wall, flexion at

Root.	Movement.
	hip, with adduction of thigh, extension at knee, slight rotation outwards of thigh.
5th Subthoracic ..	Flexion at hip, with adduction of thigh, extension at knee, drawing up of inner edge of foot, with slight flexion at ankle, and slight extension of hallux.
6th " ..	Extension at hip, with adduction of thigh, flexion at knee, flexion at ankle, lifting of outer edge of foot, extension of toes, with adduction of hallux.
7th " ..	Extension at hip, flexion at knee, extension at ankle, tilting of outer edge of foot, flexion of digits, with strong adduction of hallux, depression of root of tail, slight rotation outward of the thigh.
8th " ..	Slight rotation outward of the thigh, extension at hip, flexion at knee, extension (very strong) at ankle, strong flexion and adduction of hallux, flexion of digits in "interosseal" position, contraction of anus, root of tail depressed and drawn to side stimulated.
9th " ..	Slight rotation outwards of the thigh, flexion of digits, perineum pushed down, contraction of anus, abduction of root of tail toward side stimulated.
10th " ..	Abduction of root of tail toward side stimulated.
11th " ..	Proximal two-thirds of tail drawn toward side stimulated.
12th " ..	Distal half of tail drawn toward side stimulated.
13th " ..	Tip of tail drawn toward side stimulated.

Post-axial Class.

1st Subthoracic ..	Quadratus lumborum, psoas parvus, external oblique, internal oblique, transversalis.
2nd " ..	Quadratus lumborum, psoas magnus, cremaster, external oblique, internal oblique, transversalis.
3rd " ..	Psoas magnus, cremaster, iliacus, external oblique, transversalis (the lower part only

Root.	Movement.
	of the three latter), pectineus, adductor longus, sartorius.
4th Subthoracic ..	Psoas magnus, iliacus, pectineus, the adductor longus, gracilis (probably the rest of the adductor mass), sartorius, vastus internus, vastus externus, crureus, rectus fem. (slight), obturator externus.
5th „ ..	Gracilis, adductor longus (slight), tensor vaginæ femoris, rectus femoris, vastus internus, vastus externus, crureus, tibialis anticus, peroneus longus, semimembranosus (these latter only slightly), extensor hallucis (very slight).
6th „ ..	Part of adductor magnus, tibialis anticus, extensor longus digitorum, extensor hallucis, peroneus longus, peroneus brevis, short (intrinsic) extensors of the digits, abductor minimi digiti, gastrocnemius (both heads, but slight), popliteus, tibialis posticus, flexor longus digitorum, long flexor of the hallux, soleus, semimembranosus, plantaris, semitendinosus, biceps.
7th „ ..	Adductor magnus, semitendinosus, semimembranosus, tibialis anticus, extensor longus digitorum, extensor propr. hallucis, peroneus brevis, peroneus longus, plantaris, popliteus, gastrocnemius (both outer and inner heads), tibialis posticus, flexor longus digitorum, soleus, long flexor of the hallux, short extensor (intrinsic extensor) of the digits and hallux, short and accessory flexors (intrinsic flexor) of the digits and hallux, abductor minimi digiti, the abductor hallucis, the adductor hallucis, large part of pyriformis, interossei and lumbricales, obturator internus, quad. fem. and two gemelli.
8th „ ..	Biceps, semimembranosus, semitendinosus, gluteus medius, gastrocnemius (both heads), soleus, tibialis posticus, flexor longus digitorum, abductor hallucis, abductor natum, short and accessory flexor of the digits and hallux, adductor of the hallux, interossei and lumbricales, sphincter ani, sphincter vaginæ, small part of pyriformis, obtu-

Root.	Movement.
	rator internus, quad. fem. and the two gemelli.
9th Subthoracic ..	Short flexor of the digit and hallux, adductor of the hallux, interossei and lumbricales, sphincter vaginæ, obturator internus, sphincter ani.

The results of the experiments are in harmony with those of Eckhardt. Many of the muscles of the limb are supplied by three spinal roots, some by two; one alone, as far as I have yet observed, by a single root only. Individual variation is frequent. Excitation of the same spinal root not always throws into action the same muscles, even in individuals of the same species, sex, and approximate age; nor does it always produce the same movement, *e.g.*, *flexion* at knee followed excitation of 5th root in two individual instances. Analysis of the distribution of the component filaments of a root shows that in different individuals filaments which correspond in absolute position in the nerve-root do not correspond in function. Nevertheless, Herringham's "Law I" (quoted above) holds good for the outflow of fibres throughout considerable regions of the cord, although a sciatic plexus of the post-axial class may occur in the same individual as a brachial plexus of the pre-axial class, so that in its narrowest sense the "law" is not always applicable to great lengths of the cord. No exception has been found to it in the sense that an efferent fibre pre-axial in one individual to some particular other efferent fibre is ever in any individual of the same species post-axial to it.

The distribution of the peripheral nerve-trunks is not obviously different, whether by its root-formation the plexus belong to the pre-axial or to the post-axial class. The peripheral nerve-trunks are, as regards their muscles, relatively stable in comparison with the spinal roots. When the innervation of the limb-muscles is of the pre-axial class, so also is that of the anus, vagina, and bladder; and conversely.

The region of outflow from the spinal cord of the fibres destined for a natural group of the limb-muscles, or the representation of a particular movement at a limb joint, is often not conterminous with the origin of the filaments of a spinal root, but has its limits at points within spinal segments, either overstepping or falling short of their boundaries. Thus the outflow to the intrinsic muscles of the sole sometimes has its upper limit placed nearly midway up the region of origin of the filaments of the 6th root. The lower limit of the outflow to the calf muscles sometimes lies about two-thirds down the region of origin of the 8th root. Other examples could be cited. The ankle, knee, &c., which seem to be divisions between funda-

mentally distinct portions of the limb, are not regarded as such in the segments of the spinal cord.

If the simple movements (flexion, &c.) of the limb-joints be considered individually, the region of representation in the spinal roots of *Macacus* extends for each into at least three segments of the cord. The region of representation for each simple movement is about as extended for the small joints (digits) as for the large (hip-knee). The whole region of representation for the movements of the knee is, however, longer (includes more cord segments) than that for the ankle; and that for the hip is longer than that for the knee. This is because the more distal the joint the greater the overlap of the regions of representation in the roots of each of the two opposed movements at the joint. Of the opposed movements, the one which is in a direction toward the anterior aspect of the limb is always represented the more pre-axially in the spinal roots.

In the thigh, the nerve-roots supplying the musculature are none of them common at once to the muscle groups of the anterior and posterior aspects of the thigh. In the foot and leg the nerve-roots supplying the muscles each supply muscles situated both on the anterior and posterior aspects; this is more marked in the case of the foot than of the leg; yet in the former even the musculature of the sole is distinctly post-axial to that of the dorsum.

Although there is clear evidence that the nerve supply of the skin of the hallux is pre-axial to that of the 5th digit, my experiments have given only equivocal evidence that the musculature of the hallux is pre-axial to that of the minimus; nor is in the thigh the gracilis (lower part) pre-axial to the vastus externus. The mutual relationship of gracilis and vastus externus is as that of rectus abdominis to erector spinæ in the trunk, *i.e.*, ventral to dorsal; the same is probably true of hallux and 5th digit (as regards their musculature). This is in accord with Paterson's* views of the mutual relationship of the obturator and anterior crural nerves, although not with his extension of a similar view to the relationship of the internal and external popliteal nerves.

The posterior aspect of the thigh and leg afford an important exception to the rule given by Forgue and Lannegrace, and confirmed, as regards the fore-limb, by Herringham, viz., that, of the superficial and deep muscular layers of a region of the limb, the superficial layer is innervated by more pre-axial roots than the deep layer. The reverse holds good for the calf muscles and the hamstrings.

The significance of the distribution of the efferent fibres of a spinal root is, as J. Müller suggested, anatomical (based on metamerism, &c.) rather than functional (based on co-ordinate action, &c.). Excitation of an entire efferent root produces a combined movement

* 'Jl. of Anat. and Physiol.,' 1887 and 1889.

due to the action of many muscles, but there is no safe ground for believing that the combination is of a functional character; the weight of evidence is against this.

As to the question whether a muscle, when supplied by several nerve-roots, is supplied by them in such a way that one piece of the muscle is supplied by one root, another by another, although there is certainly great interlapping of regions belonging to the individual roots, I cannot agree with Forgue and Lannegrace when they say, "Excitation of a spinal root determines in the muscles which it supplies a total, not a partial, contraction." Simple inspection is enough to convince one, that in the case of some of the larger muscles, *e.g.*, in the thigh and spinal regions, the nerve supply from the individual roots is distinctly partial, that a district of the muscle belongs to this root, another district to that, although always with a large mutual overlap; striking examples are given by the *sartorius*, 3rd and 4th (*Macacus*) *sacrococcygeus superior*, 7th, 8th, 9th (cat), &c. On the other hand, as the distal end of the limb is approached, the intermingling of the root-districts in the several muscles becomes more intimate, and in the muscles of the sole the intermingling of the muscle-fibres belonging to individual nerve-roots is so complete as to baffle analysis, except by the degeneration method. In the sphincter muscle of the anus there is an overlap of the motor distributions of the right and left halves of the body. The sphincter ani is supplied by four nerve-roots, two right-hand, two left-hand. Any three of these may usually be cut through without the anus becoming patulous, or exhibiting asymmetry. Conversely, excitation of any one of the efferent roots supplying it causes contraction of both right and left halves of it. The innervation of the bladder from its right- and left-hand roots, is, on the other hand, neither in the case of its sympathetic nor its direct spinal supply of a bilateral character.

IV. "On the Causation of Diphtheritic Paralysis." By SIDNEY MARTIN, M.D., F.R.C.P., Assistant Physician to University College Hospital. Communicated by GEORGE BUCHANAN, M.D., F.R.S. Received March 2, 1892.

The paralysis following diphtheria in man is so closely associated with the acute disease that it is more correctly considered as a symptom and not a sequela. Its mode of production in man has not been demonstrated.

A chemical examination of the blood and spleen of eight patients who had died of diphtheria revealed the presence of two classes of substances not normally present in the tissues of the body, viz. (1) of

two albumoses or digested proteids, proto- and deutero-albumose, giving the same chemical reactions as the albumoses of peptic digestion, and (2) of an organic acid, which is soluble in absolute alcohol and in water, to a less extent soluble in amyl alcohol, and insoluble in ether, chloroform, or benzene. There is no base or alkaloid present. Owing to the small quantities in which this acid was obtained, a more detailed chemical examination was not possible.

Physiological Action of the Albumoses.—When injected into the circulation of a healthy rabbit these albumoses produce fever. If a single dose only be given, the fever subsides, and the animal remains apparently well for months. A single dose, however, may kill in a few hours.

Repeated doses of the albumoses, besides producing fever, cause a paralysis which may come on in two days, but more often is evident in six or seven days, and may be delayed for twenty days if the dose is small.

The total doses given were between 0·083 gram and 0·157 gram per kilo. of body weight in rabbits weighing between 1000 and 2000 grams.

The paralysis is not complete, but is a paresis, and is not accompanied by any special wasting of the paralysed parts. The paralysis is progressive, and, if the dose be large enough (over 0·1 gram per kilo. of body weight), the animal dies in syncope with either slow or quickened respiration.

The animals that do not die, but show paralysis, may have syncopal attacks, with an affection of the respiration; but they recover from these.

Five animals were used for experiment, and they all showed the same symptoms, including a loss of body weight, which is proportional to the dose of the albumoses.

A *post-mortem* examination of these animals showed that the blood was slow in coagulating with the largest doses. Bacteria were absent from the blood and tissues, and in only one case was any œdema (of the abdominal wall) found.

After staining with osmic acid and counterstaining with borax-carmine, the nerves were found extensively degenerated, while the spinal cord, spinal ganglia, and brain were normal.

The degeneration of the nerves is what has been described by Gombault* in his experiments on lead poisoning as "un névrite-segmentaire périaxile," or a segmental degeneration.

This degeneration affects a segment of the nerve; the fibres at that part lose their white substance of Schwann, and the axis cylinders become attenuated, and, in many cases, ruptured. If the axis cylinder

* 'Archives de Physiologie,' 1880–81, p. 11.

becomes ruptured, the nerve fibre below the point undergoes the Wallerian degeneration. The early stage of the segmental degeneration is the breaking up of the white substance of Schwann.

There may be more than one degenerated segment in the nerve which may then undergo completely the Wallerian degeneration. Above the degenerated segment the nerve is normal, the change being simply peripheral and not central in origin.

All nerves in the body may be affected by this degeneration: the motor nerves, the sensory, and the visceral (sympathetic).

An example may be quoted to show the extent of the nerve change. A rabbit, which received two doses, equal to 0·1 gram per kilo. of body weight, showed definite palsy on the twentieth day, and was killed on the twenty-fourth.

Segmental degeneration was found in the following nerves :—

I. Motor.

Of leg.

Nerve to sartorius.
,, vastus.
,, sehimimembranosus.
,, semitendinosus.
,, biceps.
,, gastrocnemius.

Of arm.

Nerve to pectorales.
,, triceps.
,, biceps.
,, flexor of forearm.

Of diaphragm.

Phrenic.

Of laryngeal muscles.

Left recurrent laryngeal.

Of psoas.

Of eye muscles.

Branches of third cranial nerve.

II. Sensory Nerves.

Long saphenous nerve.
Cutaneous thoracic nerve.

III. Visceral.

The lower part of right cervical sympathetic.

The nerve change is, therefore, widely spread over the body.

Physiological Action of Organic Acid.—This is much less toxic than the albumoses, and I have not succeeded in producing paralysis with it. It, however, produces a moderate degree of nerve degeneration when injected into the circulation.

The nerve degeneration is associated with a fatty degeneration of the muscles, which is proportional to the degree of degeneration. The heart, in all cases, shows advanced fatty degeneration.

Diphtheritic Membrane.—The membrane in diphtheria consists chemically of fibrin, hetero-albumose, proto-, and deutero-albumose, *i.e.*, it is in a state of digestion. From it was obtained an extract which was 3—5 times as toxic as the albumoses removed from the body, producing the same symptoms (fever, paralysis) and the same nerve degeneration.

This poison is probably the same as that isolated by Roux and Yersin, and is presumably of a ferment nature, the albumoses and organic acid found in the bodies of the patients being the result of the action of the ferment on the proteids of the tissues.

Diphtheria would, therefore, be, from this point of view, a disease in which the *Bacillus diphtheriæ* growing in the membrane excretes a ferment which, being absorbed, digests the proteids of the body, with the formation of albumoses and an organic acid, the action of the former of which is to produce fever and paralysis dependent on nerve degeneration.

Presents, March 17, 1892.

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Fourteen *Carte de Visite* Photographs of Fellows of the Royal Society. Messrs. Maull and Fox.

March 24, 1892.

Mr. JOHN EVANS, D.C.L., LL.D., Treasurer and Vice-President, in the Chair.

The Right Hon. Spencer Compton Cavendish, Duke of Devonshire, was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Croonian Lecture was delivered as follows:—

CROONIAN LECTURE.—"The Temperature of the Brain, especially in relation to Psychical Activity." By ANGELO MOSSO, Professor of Physiology in the University of Turin. Received March 24, 1892.

(Abstract.)

In his investigations on the temperature of the brain the author has employed, in preference to the thermo-electric pile, exceedingly sensitive mercurial thermometers, constructed specially for the purpose. Since each thermometer contains only 4 grams of mercury, the instruments respond very rapidly to changes of temperature, and a change of not more than 0·002° C. can easily be measured by means of them. The author has studied the temperature of the brain, comparing it with that of arterial blood, of the muscles, of the rectum, and of the uterus; his observations were made on animals under the influence of morphia or various anæsthetics, and also on man.

The curves of the observations made show that in profound sleep a noise, or other sensory stimulus, is sufficient to produce a slight development of heat in the brain, without the animal necessarily awakening.

In profound sleep the temperature of the brain may fall below that of the blood in the arteries. This is due to the very great radiation of heat which takes place from the surface of the head.

The brain when subjected to the action of the ordinary interrupted current rises in temperature. The rise is observed earlier in the brain than in the blood, and the increase is greater in the brain than in the general blood-current or in the rectum. During an epileptic seizure brought on by electrical stimulation of the cerebral cortex,

the author observed within twelve minutes a rise of 1° C. in the temperature of the brain.

As a rule the temperature of the brain is lower than that of the rectum; but intense psychical processes, or the action of exciting chemical substances, may cause so much heat to be set free in the brain that its temperature may remain for some time 0·2° or 0·3° C. above that of the rectum.

When a dog is placed under the influence of curare, the temperature of the brain remains fairly high, while that of the muscles and that of the blood falls. The difference of temperature thus brought about is great and constant. In one instance, the temperature of the brain was 1·6° C. above that of the arterial blood in the aorta. Such observations warn us not to regard the muscles as forming, *par excellence*, the thermogenic tissue of the body.

In order to show how active are the chemical processes in the brain, it is sufficient to keep the animal in a medium whose temperature is the same as that of the blood. When the effects of radiation through the skull are thus obviated, the temperature of the brain is always higher than that of the rectum, the difference amounting to 0·5° or 0·6° C.

Observations made while an animal is awake tend to show that the development of heat due to cerebral metabolism may be very considerable, even in the absence of all intense psychical activity. The mere maintenance of consciousness belonging to the wakeful state involves very considerable chemical action.

The variations of temperature, however, observed in the brain, as the result of attention, or of pain or other sensations, are exceedingly small. The greatest rise of temperature observed to follow, in the dog, upon great psychical activity was not more than 0·01° C. When an animal is conscious, no change of consciousness, no psychical activity, however brought about experimentally, produces more than a slight effect on the temperature of the brain.

The author shows an experiment by which it is seen that, as part of the effect of opium, the brain is the first organ to fall in temperature, and that it may continue to fall for the space of eighteen minutes, while the blood and the vagina are still rising in temperature.

The author discusses the elective action of narcotics and anæsthetics. He shows that these drugs suspend the chemical functions of the nerve-cells. In a dog rendered completely insensible by an anæsthetic, one no longer obtains a rise of temperature upon stimulating the cerebral cortex with an electric current. These results cannot be explained as merely due to the changes in the circulation of the blood. The physical basis of psychical processes is probably of the nature of chemical action.

In another experiment, in an animal rendered insensible with

chloral, the curves of temperature show that when the muscles of a limb are made to contract, the temperature of the muscles rises, but falls rapidly as soon as the stimulation ceases, soon returning to the normal. This is not the case, however, with the brain excited by an electric current. Here the stimulus gives rise to a more lasting production of heat; the temperature may continue to increase for several minutes after the cessation of the stimulation, indeed often for half an hour. This may possibly explain why, upon an electric stimulation of the cerebral cortex, the epileptiform convulsions are not immediately developed, but only appear after the lapse of a latent period of several minutes.

This experiment may be made to show the elective action exercised upon the brain by stimulant remedies. The injection of 10 centigrams of cocaine hydrochlorate produces a rise of temperature in the brain of 0·36° C., without any change in the temperature of the muscles or of the rectum being observed. In a curarised dog, the intervention of the muscles being thereby excluded, the action of the cocaine may produce a rise of as much as 4° C. in the temperature of the brain, the author having observed a rise from 37° to 41° C. This shows that in arranging the calorific topography of the organism a high place must be assigned to the brain.

The author concludes by expressing the hope that the comparative study, by the direct thermometric method, of the temperature of the various organs of the body will enable us to push forward our knowledge of the phenomena of life.

Presents, March 24, 1892.

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Bronze Medallion, 7 inches diameter, cast in honour of Professor Rudolph Virchow, For. Mem. R.S.

Virchow Testimonial Committee, Berlin.

March 31, 1892.

Mr. JOHN EVANS, D.C.L., LL.D., Treasurer and Vice-President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

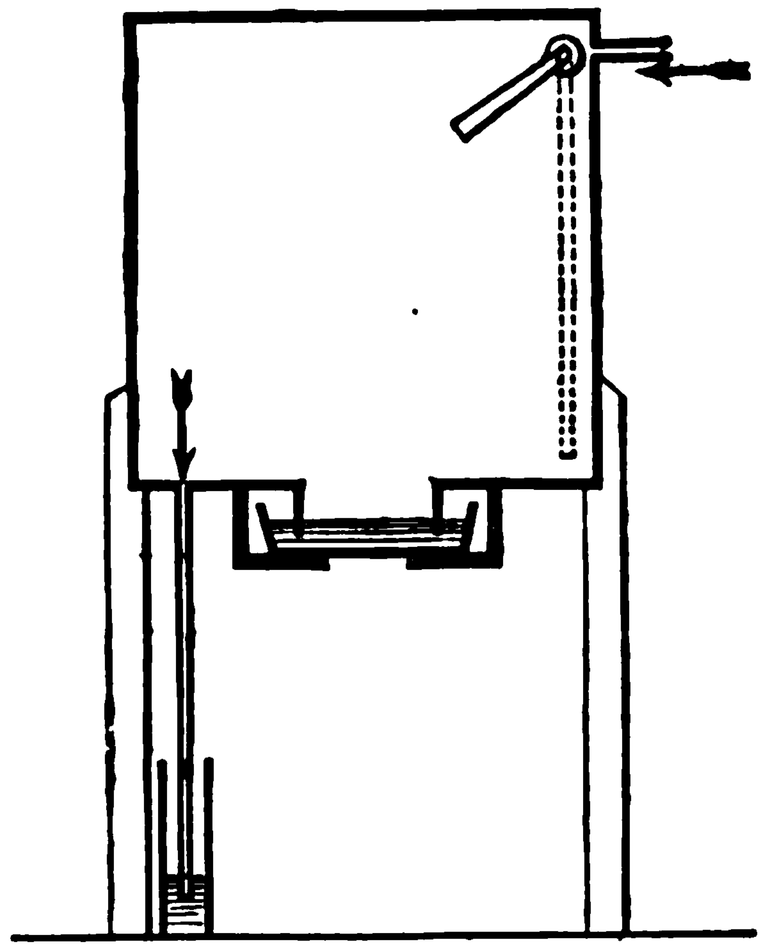
The following Papers were read:—

I. "An Improved Apparatus for ascertaining the Sensitiveness of Safety-lamps when used for Gas-testing." By FRANK CLOWES, D.Sc. (Lond.), Professor of Chemistry, University College, Nottingham. Communicated by Professor ARMSTRONG, F.R.S. Received March 24, 1892.

An apparatus devised for the purpose of testing the sensitiveness of different forms of miners' safety-lamps, when they are employed for detecting and measuring low percentages of firedamp, has been already described in the 'Roy. Soc. Proc.' (vol. 50, 1891, p. 122).

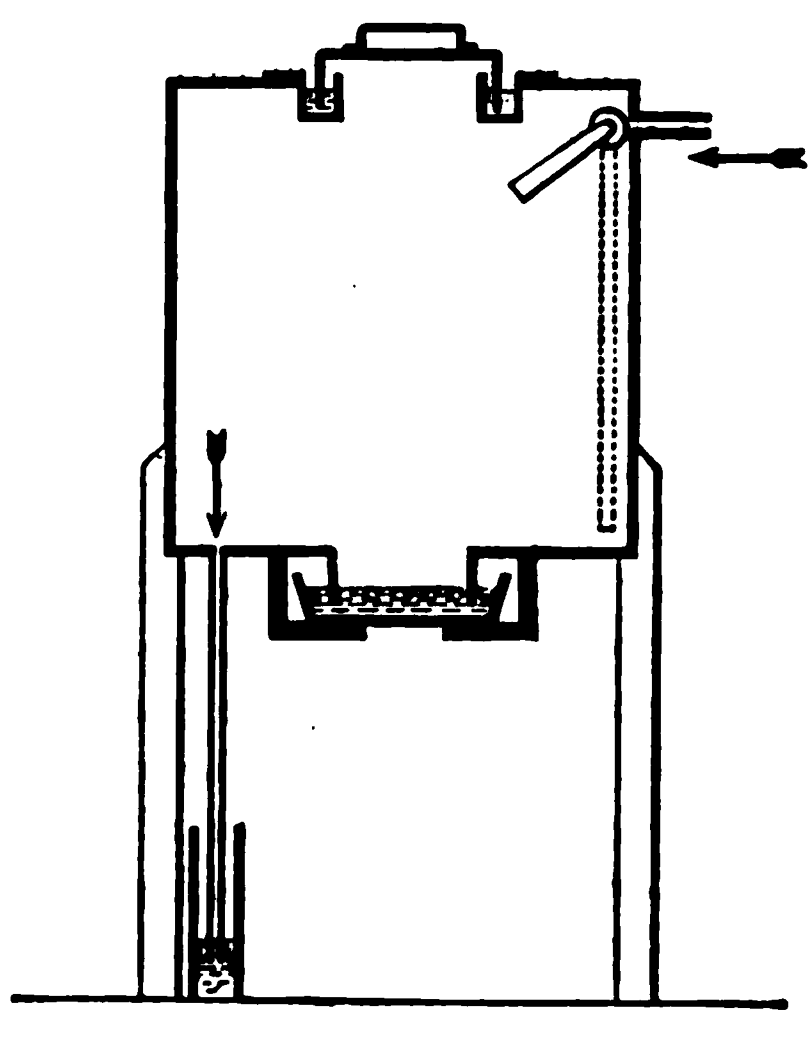
The section of the apparatus (fig. 1) shows that the apparatus consists of a large cubical wooden box or chamber, into the upper part of which the requisite measured volume of methane or firedamp can be introduced, the complete admixture of this gas with the air of the chamber being then secured by swinging up and down a broad wooden flap, the area of which is nearly equal to the square section of the chamber.

Fig. 1.



The only inconvenience and delay experienced in making a series of tests in this chamber, with varying mixtures of gas and air, arose from the difficulty in rapidly, and with certainty, replacing the mixture in the chamber by fresh air. This was attempted at first by blowing air from bellows through the opening in the floor of the chamber. But the process was tedious and uncertain, because the

Fig. 2.



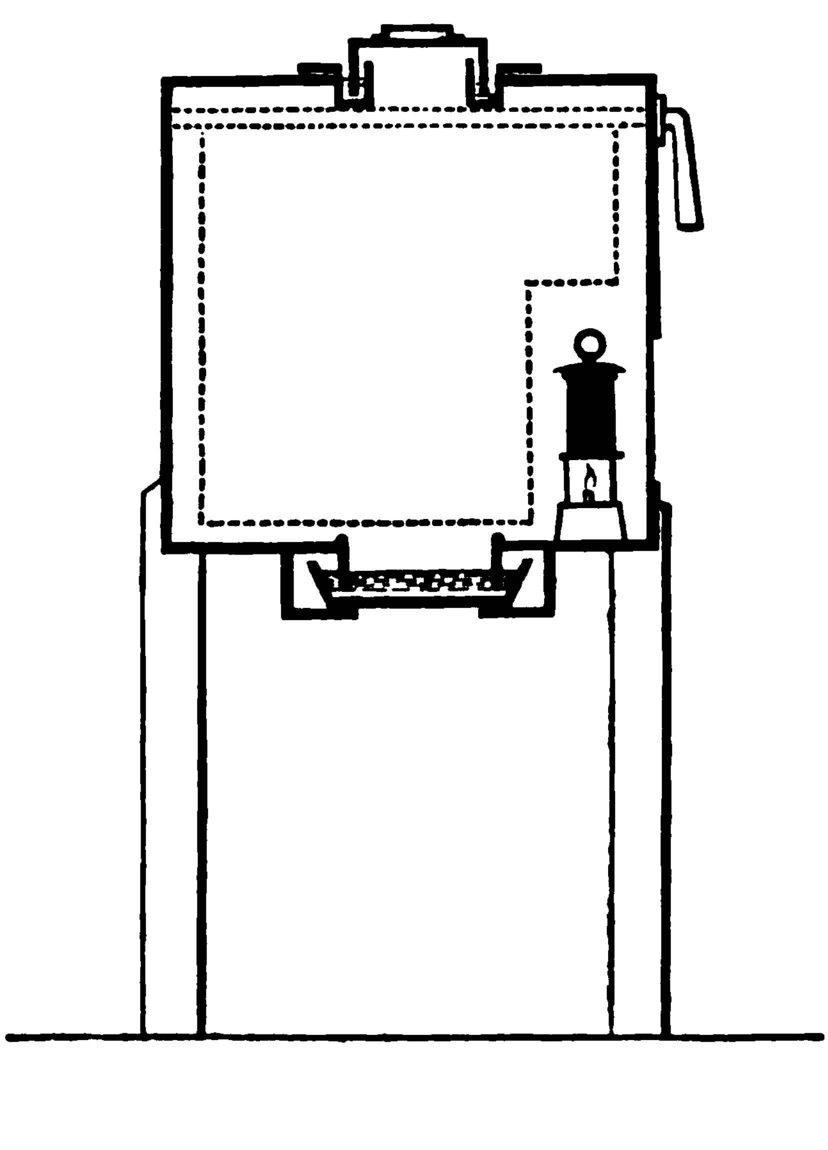
gases heated by the burning of the lamps tended by their lightness to remain near the roof of the chamber, and were, therefore, only gradually removed by a process of dilution with fresh air.

The renewal of the atmosphere in the interior of the apparatus is now effected by a much more certain and simple means.

An opening is made in the roof of the chamber, immediately over the large aperture in the floor, and corresponding to it in size. This upper opening is closed with a water-seal, as in the case of the lower one. To effect this a square trough of sheet zinc, about 2 inches in depth, is fitted closely against the edge of the aperture, and a weighted deep-edged lid dips into water, which nearly fills this trough. By bedding a broad flange of this trough with red lead upon the top of the chamber, screwing down broad wooden strips upon the flange, and brushing the surface over with melted paraffin-wax, an air-tight joint was at once obtained (see fig. 2).

The replacement of the atmosphere in this modified chamber is effected by removing the water-seals from both the upper and the lower openings. The chamber, after having been used in an experiment, is then found to be filled with fresh air after simply being allowed to stand for a few minutes. The replacement of the atmosphere within the chamber may be secured in the course of a few seconds by swinging the flap up and down several times. By this means not only is the introduction of the fresh air much more rapidly effected than by the use of bellows, but the complete removal of the gaseous mixture filling the chamber is secured with certainty.

FIG. 3.



[Increased accuracy has been obtained in the recent experiments made with the hydrogen lamp, hereafter described, in the test chamber by the removal of a small part of the mixing flap (see fig. 3). This renders it possible to place the lighted safety-lamp in the chamber before the gas is introduced and mixed with the air; since it enables the flap to be swung within the chamber without touching the lamp. Accordingly the test is commenced by placing the lamp in position close to the glass front; the chamber is then closed, the measured volume of gas is introduced and mixed with the air by moving the flap, and the "cap" is observed, and its appearance noted as soon as it undergoes no further change. The chamber is only opened finally when the lamp is to be removed. Any slight alteration in the gaseous mixture, which was formerly caused by the subsequent introduction of the lamp, is thus avoided.—March 26, 1892.]

II. "On the Application of a Hydrogen Flame in an ordinary Safety-lamp to the Detection and Measurement of Firedamp." By FRANK CLOWES, D.Sc. (Lond.), Professor of Chemistry, University College, Nottingham. Communicated by Professor ARMSTRONG, F.R.S. Received March 24, 1892.

In a former paper ('Roy. Soc. Proc.,' vol. 50, p. 122) an apparatus was described in which the appearance of the "cap" over the flame of a safety-lamp could be observed and measured when the lamp was exposed to definite mixtures of air with methane or firedamp. The relative sensitiveness of different forms of lamp, and of different flames, when they are applied to the detection and measurement of "gas," was thus readily ascertained. It was stated that the flames of colza oil, rape oil, mixed oils, benzoline, methylated spirit, and hydrogen had been experimented upon: and that the non-luminous flames producible by benzoline, alcohol, and hydrogen far excelled the more or less luminous oil flames in their power of indicating low percentages of inflammable gas or vapour in the air. It was further found that the delicacy of the test was much increased by grinding the inner surface of the back of the glass cylinder of the lamp so as to destroy its reflecting power.

Ashworth's modified benzoline safety-lamp was especially referred to as an efficient lamp both for lighting and for gas-testing. The brilliant illuminating flame gave a forward light equal to one miner's candle. When it was reduced in size by drawing down the wick it became blue and non-luminous: and when it was viewed in this condition against the ground glass surface, or, better still, against the dead-black background produced by smoking the interior of the

lamp glass at the back, a distinct flame cap, 7 mm. in height, was seen in air containing only 0·5 per cent. of methane or fire-damp. The height, density, and definition of the cap over this flame were found to increase pretty regularly as the percentage of "gas" in the air was augmented. The efficiency of this lamp for lighting and testing is thus placed beyond doubt.

In continuing the experiments described in the former paper, however, a comparison of the "caps" produced by the flame of this lamp with those produced by a small alcohol flame and a small hydrogen flame showed that the latter flames were more sensitive as gas indicators than that of the benzoline lamp. Thus, when an alcohol and hydrogen flame, each 10 mm. in height, were introduced, together with the small blue benzoline flame, into the testing chamber, which was filled with air containing 1 per cent. of coal gas, flame caps of the following dimensions were obtained:—

Hydrogen............	27	mm.
Alcohol..............	19	,,
Benzoline............	7·2	,,

It is true that the benzoline flame employed in this experiment was much smaller than the two other competing flames. But it must be remembered that the benzoline flame is *necessarily* small when it is employed for gas-testing, since, if its dimensions are increased, it becomes luminous, and renders the pale "cap" invisible. And one of the principal advantages of the alcohol and hydrogen flames consists in their remaining non-luminous, even when they are made of large dimensions; the greater surface and higher temperature of their larger flames producing, therefore, much larger and more visible "caps" than is possible with a small benzoline flame.

It will be seen from the results of the experiment just described that the hydrogen flame has the advantage over the alcohol flame in the dimensions of the "cap" which it yields. But by prolonging the test, another advantage of the hydrogen flame over its rival was ascertained. The two flames were allowed to burn side by side in the chamber, charged with air containing 1 per cent. of coal gas, for over thirty minutes. Throughout this protracted test both the hydrogen flame and the "cap" above it remained unaltered in size and appearance. The alcohol flame and its "cap," however, steadily diminished in size: after five minutes the height of the cap had fallen from 19 mm. to 12·5 mm., and after another interval of five minutes the height of the cap was reduced to 6·5 mm.: and thirty minutes after the beginning of the experiment the flame was spontaneously extinguished. This result seems to indicate that the alcohol flame is much more sensitive to the influence of the presence of products of combustion, and to deficiency of oxygen, than the hydrogen flame

is: the difference is possibly due to the much smaller quantity of oxygen required by the hydrogen flame for its combustion.

Alcohol Lamps.—The possibility of producing large and distinct "caps" when testing by means of an alcohol flame for low percentages of "gas" in air was taken advantage of by G. Pieler in 1883 to produce the most sensitive form of safety-lamp for gas-testing yet invented. The Pieler lamp is a large Davy lamp, burning alcohol from a circular wick, which yields a large flame, 30 mm. in height. When the lamp is used for testing, even the feeble light of this flame is shielded from the eye, so as not to interfere with the perception of the "cap." Experiments with this lamp in known percentage proportions of "gas" and air have been described by several observers, and their results were fully confirmed by a series carried out in the test-chamber. The "caps" obtained were as follows:—

0·25	per cent. of methane	gave a	30	mm. cap.
0·5	"	"	65	"
0·75	"	"	75	"
1·00	"	"	90	"

Indeed the flame of this lamp is so sensitive that when the proportion of methane in the air reaches 1·75 per cent. the "cap" touches the top of the gauze of the lamp; and with a somewhat higher percentage of "gas," the enlarged "cap" completely fills the interior of the gauze.

Practical men, who have used this lamp, seem to feel some doubt as to its perfect safety in the mine. But their main objection is that the alcohol flame is non-luminous, and accordingly a second lamp must be carried for lighting purposes when the Pieler lamp is employed for gas-testing.

Attempts have been made to obviate the inconvenience of carrying two lamps, by constructing a safety-lamp with two reservoirs. One of these would contain oil, and the other alcohol. Each would be supplied with a wick in the ordinary way. By raising or lowering one wick or the other, a luminous oil-flame for lighting purposes, or a non-luminous alcohol-flame for gas-testing, should thus be obtained at will within one and the same lamp: the flame being passed from one wick to the other, as may be required. Practical difficulties arose in the use of this composite lamp, which have prevented its adoption.

Hydrogen Lamps.—Pieler was aware of the advantage secured by employing a hydrogen flame for gas-testing. Owing to the difficulty, however, of adapting a hydrogen supply to a portable safety-lamp, he recommended that samples of air from the mine should be brought to the surface, and tested by a hydrogen flame burning from a

chemical generator, the apparatus being a fixture in a testing room situated near the pit's mouth.

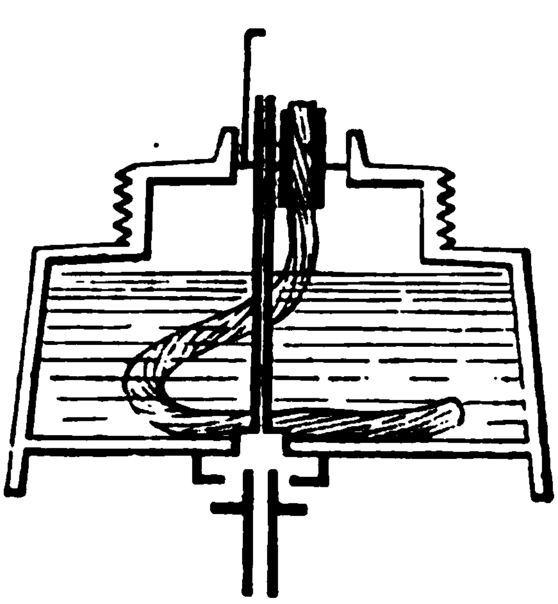
Since, however, hydrogen gas is now obtainable at small cost, compressed in light steel cylinders, it has been found possible to furnish a supply of the gas to a jet inclosed in an ordinary safety-lamp.

The lamp is so constructed that oil or benzoline may be burnt in the ordinary way from a wick when the lamp is to be used for lighting purposes, the wick being drawn down so as to produce a non-luminous flame, and this yielding caps in gas-testing when proportions of gas exceeding 3 per cent. have to be looked for. When proportions of gas less than 3 per cent. are to be looked for and estimated, the hydrogen gas is introduced by a metal jet fixed close to the wick; the gas at once kindles. The wick is then drawn down until the oil flame is extinguished, and the cap is looked for over the hydrogen flame. A cap is easily seen over this hydrogen flame when only 0·25 per cent. of gas is present. When the illuminating flame is required the wick is again pushed up, and kindled by the hydrogen flame. The supply of hydrogen may then be withdrawn. The oil flame and the hydrogen flame are thus made to supplement one another in the same lamp in gas-testing.

The hydrogen is at present supplied from a small steel cylinder, 3 inches in diameter by 8 inches in length, and weighing 4 pounds. The cylinder when fully charged contains 4 cubic feet of hydrogen. This supply lasts for many tests, since when the flame is kept burning *continuously* at the ordinary height of 10 mm. it consumes only 1 cubic foot of gas in about four hours. A regulator may be adapted to the neck of the cylinder, but it has been found that a very delicate valve in the neck of the cylinder serves to adjust the stream of gas with ease without the intervention of any regulator.

The cylinder is slung in a leather case by a strap across the shoulder. A flexible tube from the nozzle of the bottle connects the

FIG. 1.



bottle with a little flanged metal nozzle: and this can be attached to an opening in the bottom or side of the oil reservoir of the lamp. The hydrogen is thus supplied to a copper tube of fine bore which passes through the reservoir and terminates on a level with, and close by, the top of the wick (fig. 1). Before the hydrogen is fed into the lamp, it is gently turned on and allowed to sweep the air out of the flexible tube; connexion is then made with the lamp, and by careful adjustment of the valve of the bottle the flame is made of the required size. The aperture in the lamp for introducing the hydrogen is closed by an automatic valve when the hydrogen is not in use.

Several series of measurements of "caps" were made over the hydrogen flame of this lamp in the testing chamber. The flame was adjusted to a height of 10 mm. by viewing it through a metal diaphragm containing a hole 10 mm. in diameter and held outside the lamp; or by making its tip level with the top of an upright wire fixed inside the lamp and near the burner. A glass cylinder of extra height (90 mm.) was fitted into the Ashworth lamp, and a dead-black background was produced by smoking a broad strip of the internal surface of the back of this glass with the flame of a wax taper. The blackened glass not only gave a surface against which pale caps were easily seen, but the dead surface prevented reflections giving perplexing ghosts of the true flame. The percentage of methane in the air in these experiments varied from 0·25 to 3, and the following heights of cap represent the average of a large number of fairly concordant readings:—

With	0·25	per cent. of methane,		17	mm. cap.
„	0·5	„	„	18	„
„	1·0	„	„	22	„
„	2·0	„	„	31	„
„	3·0	„	„	52	„

In the 3 per cent. mixture the tip of the cap disappeared in the opaque metal cylinder of the lamp above the glass. The hydrogen flame therefore became useless for measuring higher percentages, unless it was much reduced in size: but at this point the oil flame is competent to take up the indications with certainty. When very low proportions of gas are to be tested for, the size of the hydrogen flame may be increased with advantage, as is described below; but no observer could fail to see the smallest cap mentioned above as being produced by the 10 mm. flame.

The advantage which may be obtained by increasing the size of the hydrogen flame, when small percentages of gas are being looked for, is shown by the results of the following experiments, made by exposing the hydrogen safety-lamp in air containing 1 per cent. and 0·5 per

cent. of coal gas respectively: the height of the cap being noted in each mixture when the hydrogen flame was first 10 mm., and then 15 mm., in height.

	Flame 10 mm.	Flame 15 mm.
1 per cent. gas	27 mm. cap	50 mm. cap.
$0\frac{1}{2}$ "	23 "	38 "

Attention is directed in the above statement to the *height* of the cap alone, but, as a matter of fact, its change in general appearance is also very noticeable as the proportion of gas is increased. Very careful observation of the hydrogen flame in air free from gas serves to detect a slender and very pale cap. When the gas in the air reaches 0·25 per cent. the cap becomes broader and pale grey in colour, but is still indefinite in outline, especially at its summit, and is seen only *above* the hydrogen flame. As the proportion of gas increases, the flame becomes strikingly sharp and pointed in outline, distinctly bluish-grey in colour, and gradually broadens and extends down the sides of the hydrogen flame, finally enclosing it altogether and encircling the jet. At the same time, the hydrogen flame itself is constantly growing in every dimension, gaining in luminosity and acquiring a rose-red tip. It is well to have watched the above changes in the test-chamber, and to have become familiar with the appearance of the hydrogen flame in different percentages of gas before the flame is used for gas-testing.

The use of the hydrogen flame for gas-testing has the advantage of rendering possible the employment of a non-luminous flame which can be immediately adjusted to any convenient size: not only may the size of the cap be thus increased at will by enlarging the flame, but it is possible to avoid the risk of losing the flame in the lamp, which is incurred by drawing down the wick very low when an oil flame is made use of for gas-testing.

III. "On the Application of the Safety-lamp to the Detection of Benzoline Vapour and other Inflammable Vapours in the Air." By FRANK CLOWES, D.Sc. (Lond.), Professor of Chemistry, University College, Nottingham. Communicated by Professor ARMSTRONG, F.R.S. Received March 24, 1892.

Since the vapour of benzoline and of petroleum spirit, when mixed with air, may become dangerously explosive and inflammable, it is found necessary to employ safety-lamps instead of naked lights to illuminate spaces which may contain such a mixture. The safety-lamp should accordingly be used in the neighbourhood of the oil

tanks in petroleum-carrying steamers, in petroleum stores, and in chambers in which processes are carried on which involve the use of light petroleum oil.

The suggestion naturally occurs, that the safety-lamps used in these places should be applied to ascertain whether the amount of inflammable vapour present in the air is sufficient to give rise to danger if it should come into contact with a naked flame; in other words, to ascertain if the space is efficiently ventilated. Experiments were accordingly undertaken to discover whether benzoline vapour would give rise to a "cap" over the flame of the safety-lamp, and if a "cap" appeared, to discover how small a proportion of the vapour could be detected in air by this flame-cap test. Since the hydrogen flame in the safety-lamp, and the benzoline flame in Ashworth's modified lamp, had been found to be most convenient for the formation of visible "caps," these lamps were employed in the experiments; and the test-chamber above described was employed for exposing the lamps to mixtures in varying proportions of benzoline vapour with air.

Since benzoline is a mixture of liquids, and is therefore not of invariable chemical composition, no attempt was made to ascertain the actual percentage of the vapour present in the air. The percentages of vapour, even if known and identical in amount, would probably have different effects when derived from different samples of benzoline, or even when derived from the same samples under different conditions. Accordingly an approximate determination only was made of the amount of further dilution with air, which an explosive or inflammable mixture might undergo, before the "cap" it produced over the safety-lamp flame ceased to be easily visible.

A large gas-holder was filled with air which had bubbled through benzoline, and was thus charged with the vapour at the ordinary temperature, in the same way as air would be charged with the vapour from the evaporation of stored benzoline. This mixture was inflammable when kindled in the open air. Varying proportions of this mixture with additional air were then prepared, and the effect was ascertained of introducing a flame into them. It was found that a mixture of 1 volume of the benzolised air with 4 volumes of air was violently explosive. When the proportion of air was increased from 4 to 7 volumes the mixture was still inflammable, and when the air was increased to 9 volumes the mixture ceased to be inflammable. Mixtures of the same benzolised air were then made in the test-chamber (pp. 87—90) with still larger proportions of air, and the appearance of the safety-lamp flames was examined in these mixtures, with the following results:—

Proportion of benzolised air to air in mixture.	Behaviour of the mixture with a naked flame.	Height of the "cap" over the hydrogen safety-lamp flame in the mixture.
1 : 4	Violently explosive.	
1 : 5	Burns rapidly, and would probably be explosive if fired in large quantity.	
1 : 6		
1 : 7		
1 : 8	Burns around a flame only.	
1 : 9	Non-inflammable.	
1 : 23	"	52 mm. "cap."
1 : 36	"	43 " " (4 " " with benzoline flame.)
1 : 72	"	31 mm. "cap."
1 : 144	"	22 " "

The results of these experiments showed that the 10 mm. hydrogen flame in the Ashworth safety-lamp will detect a quantity of benzoline vapour in air which is only 1/36th that of the explosive proportion, and 1/20th of that which is inflammable when mixed with air. The benzoline flame shows a very small but distinct "cap" when the amount of benzoline vapour is 1/9th that requisite for the production of an explosive mixture, and 1/5th that which will yield an inflammable mixture.

Further experiments of a similar kind are being made with the vapour of alcohol and of ether.

I have to acknowledge with pleasure the valuable manipulative skill and fertility of resource of W. T. Rigby, who has assisted me in carrying out the experiments with the test-chamber.

IV. "Aberration Problems: a Discussion concerning the Connexion between Ether and Matter, and the Motion of the Ether near the Earth." By Oliver Lodge, F.R.S., Professor of Physics, University College, Liverpool. Received March 31, 1892.

(Abstract.)

The paper begins by recognising the distinction between ether in free space and ether as modified by transparent matter, and points out that the modified ether, or at least the modification, necessarily travels with the matter. The well-known hypothesis of Fresnel is discussed and re-stated in modern form.

Of its two parts, one has been verified by the experiment of Fizeau, the other has not yet been verified. Its two parts are, (1) that inside transparent matter the velocity of light is affected by the motion of that matter, and (2) that immediately outside moving matter there is no such effect. The author proceeds to examine into the truth of this second part, (1) by discussing what is already known, (2) by fresh experiment.

The phenomena resulting from motion are four, viz.:—

1. Changes in direction, observed by telescope and called aberration.

2. Change in frequency, observed by spectroscope and called Doppler effect.

3. Change in time of journey, observed by lag of phase or shift of interference bands.

4. Change in intensity, observed by energy received by thermopile.

After a discussion of the effects of motion in general, which differ according as projectiles or waves are contemplated, the case of a fixed source in a moving medium is considered; then of a moving source in a fixed medium; then the case of medium alone moving past source and receiver; and, finally, of the receiver only moving.

It is found that the medium alone moving causes no change in direction, no change in frequency, no detectable lag of phase, and probably no change of intensity; and hence arises the difficulty of ascertaining whether the general body of the ether is moving relatively to the earth or not.

A clear distinction has to be drawn, however, between the effect of general motion of the medium as a whole and motion of parts of the medium, as when dense matter is artificially moved. The latter kind of motion may produce many effects which the former cannot.

A summary of this part of the discussion is as follows:—

Source alone moving produces a real and apparent change of colour; a real but not apparent error in direction; no lag of

phase, except that appropriate to altered wave-length; a change of intensity corresponding to different wave-lengths.

Medium alone moving, or source and receiver moving together, gives no change of colour; no change of direction; a real lag of phase, but undetectable without control over the medium; a change of intensity corresponding to different distances, but compensated by change of radiating power.

Receiver alone moving gives an apparent change of colour; an apparent change of direction; no change of phase, except that appropriate to extra virtual speed of light; change of intensity corresponding to different virtual velocity of light.

The probable absence of a first order effect of any kind, due to ethereal drift or relative motion between earth and ether, makes it necessary to attend to second order effects.

The principle of least time is applied, after the manner of Lorentz, to define a ray rigorously and to display the effect of existence or non-existence of a velocity potential. Fresnel's law is seen to be equivalent to extending the velocity potential throughout all transparent matter.

It is shown that a ray traversing space or transparent substances will retain its shape, whatever the motion of the medium, so long as that motion is irrotational, and that in that case the apparent direction of objects depends simply on motion of observer; but, on the other hand, that if the earth drags with it some of the ether in its neighbourhood, stellar rays will be curved, and astronomical aberration will be a function of latitude and time of day.

The experiment of Boscovich, Airy, and Hoek, as to the effect of filling a telescope-tube with water, does not discriminate between these theories. For if the ether is entirely non-viscous and has a velocity potential, stellar rays continue straight, in spite of change of medium (or at oblique incidence are repeated in the simple manner), and there will be no fresh effect due to change of medium; while, if, on the contrary, the ether is all carried along near the earth, then it is stationary in a telescope tube, whether that be filled with water or air, and likewise no effect is to be expected. In the case of a viscous ether, all the difficulty of aberration must be attacked in the upper layers above the earth; all the bending is over by the time the surface is reached. It is difficult to see how an ethereal drift will not tend to cause an aberration in the wrong direction.

Of the experiments hitherto made by Arago, Babinet, Maxwell, Mascart, Hoek, and perhaps others, though all necessary to be tried, not one really discriminates between the rival hypotheses. All are consistent either with absolute quiescence of ether near moving bodies, or with relative quiescence near the earth's surface. They may be said, perhaps, to be inconsistent with any intermediate position.

Two others, however, do appear to discriminate; viz., an old and difficult polarisation experiment of Fizeau,* which has not been repeated since, and the recent famous experiment of Michelson with rays made to interfere after traversing and retraversing paths at right angles.

The conclusions deducible from these two experiments are antagonistic. Fizeau's appears to uphold absolute rest of ether; Michelson's upholds relative rest, *i.e.*, drag by the earth.

The author now attempts a direct experiment as to the effect of moving matter on the velocity of light in its neighbourhood; assuming that a positive or negative result with regard to the effect of motion on the velocity of light will be accepted as equivalent to a positive or negative result, with respect to the motion of the ether.

He gives a detailed account of the experiment, the result of which is to show that such a mass as a pair of circular saws clamped together does not whirl the ether between the plates to any appreciable amount, not so much, for instance, as a 1/500th part of their speed. He surmises, therefore, that the ether is not appreciably viscous. But, nevertheless, it may perhaps be argued that enormous masses may act upon it gravitationally, straining it so as perhaps to produce the same sort of effect as if they dragged it with them. He proposes to try the effect of a larger mass. Also to see if, when subject to a strong magnetic field, ether can be dragged by matter.

The aberrational effect of slabs of moving transparent matter is considered, also the effect of a different refractive medium.

Motion of medium, though incompetent to produce any aberrational or Doppler effect, is shown to be able to slightly modify them if otherwise produced.

The Doppler effect is then entered into. The question is discussed as to what the deviation produced by a prism or a grating really depends on: whether on frequency or wave-length. It is shown that whereas the effect of a grating must be independent of its motion and depend on wave-length alone, yet that the effect observed with a moving grating by a moving observer depends on frequency, because the motion of the observer superposes an aberrational effect on the true effect of the grating. This suggests a means of discriminating motion of source from motion of observer; in other words, of detecting absolute motion through ether; but the smallness of the difference is not hopeful.

Michelson's experiment is then discussed in detail, as a case of normal reflexion from a moving mirror or from a mirror in a drifting medium. No error in its theory is discovered.

The subjects of change of phase, of energy, of reflexion in a moving

* 'Ann. de Chim. et de Phys.,' 1859.

† 'Phil. Mag.,' 1887.

medium, work done on a moving mirror, and the laws of reflexion and refraction as modified by motion, are considered.

It is found that the law of reflexion is not really obeyed in a relatively moving medium, though to an observer stationary with respect to the mirror it appears to be obeyed, so far as the first order of aberration magnitude is concerned; but that there is a residual discrepancy involving even powers of aberration magnitude, of an amount possibly capable of being detected by very delicate observation.

The following statements are made and justified :—

(1.) The planes of incidence and reflexion are always the same.

(2.) The angles of incidence and reflexion, measured between ray and normal to surface, usually differ.

(3.) If the mirror is stationary and medium moving, they differ by a quantity depending on the square of aberration magnitude, *i.e.*, by 1 part in 100,000,000; and a stationary telescope, if delicate enough, might show the effect.

(4.) If the medium is moving and mirror stationary, the angles differ by a quantity depending on the first power of aberration magnitude (1 part in 10,000), but a telescope moving with the mirror will not be able to observe it; for the commonplace aberration caused by motion of receiver will obliterate the odd powers and leave only the even ones; the same as in case (3).

(5.) As regards the angles which the incident and reflected *waves* make with the surface, they differ in case (3) by a first order magnitude, in case (4) by a second order magnitude.

(6.) At grazing incidence the ordinary laws are accurately obeyed. At normal incidence the error is a maximum.

(7.) The ordinary laws are obeyed when the direction of drift is either tangential or normal to the mirror; and are disobeyed most when the drift is at 45°.

(8.) In general, the shape of the incident wave is not precisely preserved after reflexion in a moving medium. To a parallel beam the mirror acts as if slightly tilted; to a conical beam as if slightly curved. But either effect, as observable in the result, is almost hopelessly small.

(9.) Similar statements are true for refraction, assuming Fresnel's law.

The possibility of obtaining first order effects from general ethereal motion by means of electrical observations is considered.

V. "The Abductor and Adductor Fibres of the Recurrent Laryngeal Nerve." By J. S. Risien Russell, M.B., M.R.C.P. Communicated by Professor V. Horsley, F.R.S. Received March 17, 1892.

(From the Pathological Laboratory of University College, London.*)

CONTENTS.

Introduction.

While engaged in certain experimental investigations in connexion with the cervical nerve roots of the dog ('Roy. Soc. Proc.,' 1892), the ease with which I found one could separate, in a nerve root, the different bundles of nerve fibres which are concerned with one function from those concerned with another, or even a bundle of nerve fibres destined for the supply of one muscle from one destined for the supply of another, led me to suppose that by exercising sufficient care, it might be possible to separate, in the same way, the abductor from the adductor fibres in the recurrent laryngeal nerve. It is a matter of clinical and pathological experience (Semon, Rosenbach) that in organic and progressive affections of this nerve the abductor fibres are prone to succumb before the adductors; but why this should be so is not at all clear. In like manner, in the well-known experiments of Gad and B. Fränkel† on freezing the nerve, the abductor fibres give way before the adductors do. Jeanselme and Lermoyez,‡ by electrical excitation of the laryngeal muscles of human

* Part of the expenses connected with this experimental research have been defrayed by a grant from the Scientific Grants Committee of the British Medical Association.

† 'Centralblatt für Physiologie,' May 11, 1889.

‡ 'Arch. de Physiol. normale et pathol.,' No. 6, 1885.

subjects who died of cholera, found that the crico-arytenoideus posticus muscles were the first to lose their excitability after death. This observation has been abundantly confirmed by Horsley and Semon* for all classes of animals. On the other hand, ether, reaching the laryngeal muscles through the circulation, puts the adductors into abeyance before the abductors, so that, while excitation of the recurrent nerve of an animal not very deeply under its influence results in adduction of the corresponding vocal cord, similar excitation of the nerve when the animal is profoundly under the influence of the anæsthetic results in abduction of the cord.† It seemed, therefore, not unlikely that, if the fibres dominating over the one function could be successfully separated from those dominating over the other, some fresh light might be thrown on this subject.

It is interesting to find that Dr. Felix Semon,‡ in a paper written so long ago as 1881, asked the following question with regard to the fibres of the recurrent laryngeal nerve: "Are we to suppose that, though the nerve is apparently homogeneous, it consists in reality of a bundle of strictly differentiated fibres, bound together simply by a common nerve sheath, and actually differentiated throughout their peripheral course; in fact, having ganglionic centres of their own?" Dr. Semon then goes on to show that the pathological facts strongly support the probability of the truth of this hypothesis.§

Morell Mackenzie,|| in his text-book published the year before Semon raised this question, suggested that possibly the abductor filaments were more superficially situated than the adductor fibres, and that this, if true, would account for the proneness of the abductors to succumb before the adductors in affections, especially compression by tumours, of the nerve. There is, however, no ground for this hypothesis.

Since then, the general question of the proclivity of the abductor fibres has been the subject of great controversy, and has been debated both from a clinical and pathological standpoint,¶ but, so far as I am aware, very few attempts have been made to determine

* 'British Medical Journal,' Aug. 28 and Sept. 4, 1886.

† Hooper, 'Trans. of the Amer. Laryngol. Assoc.,' vol. 7; Horsley and Semon, 'British Medical Journal,' Sept. 4 and 11, 1886.

‡ Semon, 'Arch. of Laryngology,' 1881, vol. 2, p. 200.

§ Dr. Semon's views have been amply confirmed by direct experiments on the cortical and bulbar centres for the laryngeal muscles. See history of the subject in paper by Semon and Horsley, 'Phil. Trans.,' 1890.

|| Mackenzie, 'A Manual of Diseases of the Throat and Nose,' 1880, vol. 1, p. 440.

¶ For a full history of this, see Semon's contribution to the 'Virchow-Festschrift'; "Die Entwicklung der Lehre von den motorischen Kehlkopflähmungen seit der Einführung des Laryngoscops," 1891.

the arrangement of the fibres in the trunk of the recurrent laryngeal nerve, either by anatomical observation or direct experimental investigation.

Historical Account of Previous Experimental Researches.

Hooper,* wishing to decide, by experimental investigation, whether it is the abductor or the adductor fibres that are really the more prone to succumb in affection of the recurrent laryngeal nerve, passed a thread through the middle of one recurrent nerve, and left it *in situ* in the hope that it would act as a foreign body and excite inflammation of the nerve. At the end of a week, on inspecting the larynx, the vocal cord on the side corresponding to the injured nerve was observed not to come up to the middle line with the same "snap" on expiration as did the cord of the opposite side.

The nerve was found imbedded in a mass of inflammatory tissue, and electrical stimulation of its trunk below this point resulted in abduction of the corresponding vocal cord. When a strong current was used, stimulation of the nerve resulted in abduction of the vocal cord of the same side, and adduction of that of the opposite side. The opposite uninjured nerve was next divided, and adduction of the vocal cord on this side followed stimulation of its peripheral end. Division of the injured nerve below the point at which the thread had been inserted, and stimulation of its peripheral end, resulted in distinct outward rotation of the arytenoid cartilage of that side and an approximation of both arytenoid cartilages at the same time. All attempts to verify these results have failed, so that the experiment stands alone. Proneness of the adductors to suffer before the abductors is what this experiment seemed to point to, but the observer was unable to lay very great stress on this single experiment, positive as it seemed.

The possibility of direct injury to the adductor fibres during the process of inserting the thread into the nerve does not seem to have occurred to this observer, a possibility which is more than likely. Further, it is quite evident that the effects he obtained on the opposite cord, by stimulating the injured nerve, were due to diffusion of the electrical current to the nerve of the other side by diffusion to the vagal trunk, and so reflexly to the opposite nerve.

Onodi† exposed the muscles of the larynx in dogs, and found that at the point where the recurrent laryngeal nerve crosses the crico-arytenoideus lateralis muscle it splits into three bundles, the first of which supplies the crico-arytenoideus posticus, the second the arytenoideus transversus and crico-arytenoideus lateralis, and the third

* Hooper, 'New York Med. Journ.,' July 4, 1885.

† Onodi, 'Berliner Klin. Wochenschr.,' 1889, No. 18.

the thyro-arytenoideus externus. He passed a ligature round each branch, divided all three branches, and then stimulated each separately by an electrical current, and noted the movements of the vocal cords. The communication is an exceedingly brief one; but the observer promised to publish a full account of his experiments later. As far as I am aware, no such publication has appeared up to the present.

These observations can scarcely be said to have thrown much fresh light on the question at issue, as it is only natural to suppose that this nerve, in common with all other motor nerves, should so near its peripheral termination divide into several branches, and that stimulation of each branch of the nerve should evoke contraction of the muscle which it supplies, and thus bring about the particular movement of the vocal cord over which the muscle presides. But this does not at all decide the all-important question as to whether the nerve fibres presiding over the different functions have a separate course throughout the entire length of the nerve trunk or not.

Dionisio* performed tracheotomy, and then inserted a laryngeal "dynamometer" between the cords. This consisted of a small india-rubber ball, which communicated by means of a tube with a mercurial manometer. The height of the mercurial column during inspiration and expiration was then noted during the natural movements of the vocal cords, its position during expiration being of course higher than during inspiration. The recurrent laryngeal nerve was exposed and stimulated with an electrical current sufficiently strong to produce moderate adduction of the corresponding vocal cord, during which the mercurial column rose. When excitation was discontinued it fell again, and oscillated between the two former points. Circular pressure was then applied to the nerve, commencing with 5 grammes, and going up to 350 grammes, by gradual stages; pressure being kept up for $2\frac{1}{2}$ minutes at each stage. It was found that on stimulating the nerve after each stage there was a gradual diminution in its power of conduction, until stimulation on the proximal side of the point of pressure no longer gave any response, while stimulation on the distal side resulted in a rise of the pressure, but not nearly so great as formerly. There was a gradual fall of pressure, the inspiratory and expiratory preserving about the same ratio they bore to each other before the pressure was commenced.

Operative Procedure.

Dogs were without exception the animals used in these experiments, and in every case ether narcosis was produced and continued throughout the whole course of the experiment, at the end of which the animal was always killed by an overdose of the narcotic, except in

* Dionisio, 'Arch. Italiani di Laringol.,' January, 1892, p. 1.

the case of those instances in which it was necessary to allow the animals to live for three weeks, for the study of the degenerations which followed section of certain parts of the nerve. In the latter case the operation, which was always of the most trivial character, was performed under strict antiseptic precautions, and the small wound afterwards treated antiseptically. In every case the small wound healed by immediate union. The animals were narcotised in these, as in the other, experiments, and at the end of three weeks death was produced by means of an overdose of chloroform.

Tracheotomy was performed in every instance (except when the animal was to be allowed to live after the operation), a glass cannula being inserted into the trachea. A short rubber tube connected the free end of the cannula with a glass funnel, through which the anæsthetic was administered during the remainder of the experiment. The trachea was then completely divided transversely above the point at which the tube had been inserted. A window was cut from the upper portion of the divided trachea and raised gently, so as to give a full view of the larynx, as seen from below, without producing the least traction on the parts, which might disturb their normal play of movement.*

One or other of the recurrent laryngeal nerves was next exposed, separated from the loose connective tissue which surrounds it, and a ligature passed round the trunk of the nerve at the lower part of the neck. The nerve was then divided on the proximal (*i.e.*, bulbar) side of the ligature, and its peripheral end was separated into its component bundles of nerve fibres, round each of which a ligature was passed and secured.

In the operation for the production of control results by the degeneration method, the nerve, after being exposed and separated from the surrounding connective tissue, was placed *in situ* upon a piece of cork, in order to steady it, and one bundle of nerve fibres being carefully separated from the others which compose the nerve, a few millimetres of this bundle were excised. These operations were performed under strict antiseptic precautions, the wounds closed by continuous aseptic silk sutures, and afterwards dressed antiseptically, healing by first intention, tracheotomy not having been performed.

* In observations of this kind it is absolutely essential to view the larynx without traction on the attachments of the cords, since in the dog and the cat traction on the larynx, whether by a ligature directly attached to it, or indirectly by pulling forward the tongue, may most easily produce the appearance of either paralysis or spasm of one vocal cord where no such condition exists in reality.

Division of Subject and Analysis of Results.

A. *Division of Subject.*

The first part of the following research consists in the separation and isolation of the different bundles of nerve fibres of which the nerve trunk is composed, electrical excitation of each separate bundle, and observation of the effects produced on the vocal cords by such excitation.

Exposure of the different bundles of nerve fibres under exactly similar circumstances to the drying influence of the external air, with observation of the relative duration of vitality possessed by the different bundles, forms the second part of the investigation.

Other methods were next instituted to control the results of the foregoing, and the first of these, constituting the third part of this work, consisted in tracing by *post-mortem* dissections each bundle of nerve fibres separated in the nerve trunk to its termination in the mucous membrane or in a muscle of the larynx.

The next control method consisted in exposing the muscles of the larynx immediately after death, and direct observation of them during excitation of the separate bundles of nerve fibres, this being controlled by occasional excitation of individual muscles themselves. This forms the fourth part of the investigation. The fifth or last part of the research served as a third control method, and consisted in observations of the muscular degenerations which followed division of one or other bundle of nerve fibres in the nerve trunk, three weeks after such division.

B. *Analysis of Results.*

Section 1. *Separation and Excitation of the Individual Bundles of which the Nerve is composed.*—The separation was brought about by means of an exceedingly delicate thin-bladed knife. The divisions between the different bundles of fibres could usually be seen by the naked eye, and further guides were the minute capillary twigs which usually course on the surface of the nerve along these lines of division. When no such guides could be seen by the unaided eye, a lens was used to assist in their recognition. Great care was taken to preserve the vitality of the nerve fibres by constantly bathing them with warm normal saline solution. Each bundle was in turn raised into the air, and stimulated by means of fine platinum electrodes attached to the secondary coil of a du Bois-Reymond's inductorium, supplied by a bichromate cell. The same strength of current was used for all the bundles of nerve fibres in any given experiment, and was on an average 5000 to 7000 on Kronecker's inductorium scale; and the rate of interruption was 100 per second. The results in twelve dogs

showed that excitation of certain bundles produced no effect on the vocal cord, while excitation of others produced abduction or adduction of the vocal cord on the same side, according to the bundle of fibres stimulated; and, as far as could be ascertained, those fibres excitation of which produced abduction of the cord were situated internally to those which produced adduction, *i.e.*, the former are situated on the side of the nerve next to the trachea. But of this point it is difficult to be absolutely certain, as the difficulties of preserving the nerve in its normal position during the investigation are very great.

Section 2. Relative Vitality of the respective Bundles.—It was found that if the bundles of nerve fibres were separated, as has been already explained, and the abductor and adductor bundles of fibres thus isolated placed upon a piece of cork, and left exposed to the air of the room under exactly similar circumstances, the abductor fibres ceased to conduct impulses in response to electrical excitation long before there was the slightest sign of similar failure on the part of the adductors. The same strength of current and the same number of interruptions per second were employed in each case, with the result that the abductor fibres ceased to conduct impulses in about twenty to thirty minutes, on an average, after transverse section of the nerve and separation of its component bundles; while the adductor fibres would continue to conduct impulses well for three hours and more.*

That this death of the abductor fibres did not take place throughout the whole length of the nerve at the same time is proved by the fact that when the portion under observation ceased to conduct impulses, an effect could be often produced by stimulating some portion of the bundle situated nearer the peripheral end of the nerve, until at last even stimulation of the nerve ends in the muscle failed to produce any effect. The adductors meanwhile acted well to the original strength of stimulus, even when applied to the original seat of separation of the bundles. As is well known, if all the fibres of the recurrent laryngeal nerve be stimulated simultaneously in the adult dog, adduction of the vocal cord results; while the same procedure in the young dog results in abduction of the vocal cord. (See all previous observers from Legallois to Semon and Horsley.) In such young animals, even after separation of the different bundles of nerve fibres in the trunk of the nerve from each other to the extent of an inch to an inch and a-half, it is at first impossible to get any other effect than abduction of the corresponding vocal cord. But there comes a time when stimulation of one of the separated bundles results in abduction, while stimulation of another results in adduction. Still later

* The experiment was never continued longer than a little over three hours, as there seemed no necessity for it.

the abductor bundle ceases to produce this effect when stimulated with a moderate strength of current, and when a very strong current is used adduction of the vocal cord follows, as in the case of stimulation of the other bundle of nerve fibres. The explanation of these phenomena seems to be that in the young dog the abductor fibres are more excitable than the adductors, so that a strength of current short of that necessary to evoke action of the adductor muscles diffuses to the abductor fibres, which, being more excitable, cause abduction of the vocal cord; but that, as in the adult animal, the tendency to death of the abductor fibres is more marked than that of the adductors, so that there comes a time in the young animal when the abductor fibres have so far lost their excitability that stimulation of the adductor bundle with a current strong enough to evoke contraction of the adductor muscles, though it still diffuses to the abductor fibres, is no longer capable of exciting them, and in the end the abductor fibres lose completely their excitability, while the adductors, though relatively less excitable in the beginning, preserve their excitability, even in this case, longer than the abductors, which on their part preserve it much longer than in the adult animal.

Section 3. (Control) Tracing the respective Bundles to their Peripheral Terminations by Dissection.—Having ascertained by electrical excitation the functions subserved by the different bundles of nerve fibres in the nerve trunk, these bundles were traced *post mortem* by careful dissection to their peripheral terminations. All the bundles of nerve fibres were thus dealt with, but the present research does not make it necessary for a description to be given of any but the motor fibres. From several such dissections, taken together with the facts ascertained during the excitation experiments, it seemed almost certain that the abductor bundle of nerve fibres is situated on the inner side of the nerve, *i.e.*, next to the trachea, while the adductor bundle is situated on the outer side of the nerve. The abductor bundle may be traced to the crico-arytenoideus posticus muscle on the same side, and to it alone, none of the fibres to the adductors being contained in this bundle; while the adductor bundle supplies branches to all the adductor muscles on the same side, and to the arytenoideus.

Section 4. (Control) Direct Observation of the Abductor and Adductor Muscles in Action after Dissection.—In six dogs the nerve was exposed at the lower part of the neck, and the different bundles of fibres separated. The nerve fibres were carefully preserved by being covered with a piece of cotton wool saturated with warm normal saline solution. The animal under observation was then killed by an overdose of chloroform, the larynx quickly excised, and its muscles exposed by dissection.

By this method, on excitation of the nerve fibres not only could the movements of the vocal cords be seen, but also the contraction of the

muscles directly engaged in bringing about the movement of abduction or adduction, as the case might be. These observations showed that so perfectly could the nerve fibres concerned with the one function be separated from those concerned with the other, that stimulation of the one set evoked contraction of the abductor muscle alone, while stimulation of the other set evoked contraction of the adductor muscles alone.

Section 5. (*Control*) *Degeneration Method.*—The difficulties which attended this group of experiments were very great.

An attempt was made to separate one bundle of nerve fibres from the others, and to excise a few millimetres of it without injuring the other bundles contained in the nerve trunk, and, of course, without severing the whole nerve trunk. As can be easily understood, the ease with which damage can be done to so delicate a nerve is very great, and in the absence of any means of fixing the nerve (such as was afforded by the ligature in the excitation experiments) without producing damage to any but the bundle of fibres that were to be eliminated, the nerve trunk rolled about so freely that the task was attended by endless difficulties. It is, therefore, scarcely surprising that, out of seven experiments, the results were unsatisfactory in three instances. But those that were successful yielded such striking results that, in view of the great difficulties attending them, further multiplication of these experiments has been considered unnecessary. In one instance, the adductor fibres in the nerve trunk were successfully separated, and a few millimetres excised. As in all these experiments, the wound healed by primary union; and when the dog was killed, three weeks after operation, the autopsy revealed the following:—The adductor muscles on the side of the damaged nerve were atrophied and degenerated, while the abductor muscle of the same side was normal, as were naturally all the muscles of the opposite side of the larynx.

In two instances the abductor fibres were successfully divided without injury to the adductor fibres. During life in one case, there were feeble attempts at abduction during vigorous inspiration, and in the other there were not the slightest signs of abduction. In both cases the autopsy revealed atrophy and degeneration of the crico-arytenoideus posticus on the side corresponding to that of the injured nerve, while the adductor muscles of the same side and all the muscles of the opposite side of the larynx were normal. In all these three cases direct electrical excitation of the muscles confirmed the conclusions which had already been come to by observing their loss of function and degenerate appearance. In the fourth case, as was supposed at the time of the operation, owing to their position, the bundle of fibres divided was evidently not one of those supplying the muscles of the larynx. Three weeks after the operation the vocal

cord on that side performed its excursions perfectly normally, as did that of the opposite side. Separation of the abductor and adductor fibres was then effected in the remainder of the nerve trunk, and excitation of these bundles evoked their respective movements of the vocal cord on the same side. Finally, on post-mortem examination in this case, none of the muscles on either side of the larynx were found atrophied or degenerated. It consequently served as a gratifying control of the other degeneration experiments.

Summary and Conclusions.

The results of these experiments show clearly :—

1. That the abductor and adductor fibres in the recurrent laryngeal nerve are collected into several bundles, the one distinct from the other, and each preserving an independent course throughout the nerve trunk to its termination in the muscle or muscles which it supplies with motor innervation, a condition of things the possibility of which was suggested by Dr. Semon more than ten years ago, from the evidence of pathological facts.

2. That while in the adult animal simultaneous excitation of all the nerve fibres in the recurrent laryngeal nerve results in adduction of the vocal cord on the same side, abduction is the effect produced in a young animal by an exactly similar procedure.

3. That when the abductor and adductor fibres are exposed to the drying influence of the air under exactly similar circumstances, the abductors lose their power of conducting electrical impulses very much more rapidly than the adductors, in other words, they are more prone to succumb than are the adductors, a fact which has for long been recognised and insisted on by Dr. Semon as being the case in the human subject, and in support of the truth of which that observer has adduced so many powerful arguments.

4. That, even in the young dog, the abductor nerve fibres, though preserving their vitality much longer than in the case of the adult animal, nevertheless in the end succumb before the adductor fibres.

5. That this death commences at the point of section of the nerve, and proceeds gradually to its peripheral termination, and does not take place in the whole length of the nerve simultaneously.

6. That it is possible to trace anatomically the abductor and adductor fibres throughout the whole length of the recurrent laryngeal nerve to their termination in the one or other group of laryngeal muscles, and that these fibres appear to bear a fixed relationship to each other throughout their course, the abductors being situated on the inner side of the nerve or that next to the trachea, while the adductors are on the outer side.

7. That it is possible to so accurately separate these two sets of

fibres in the nerve trunk that excitation of either of them evokes contraction of the abductor or adductor muscles, as the case may be, without evoking any contraction whatever in the muscle or muscles of opposite function.

8. That the bundle of nerve fibres concerned with one function may be divided without injury to that concerned with the opposite function, and that such division is followed by atrophy and degeneration of the muscles related to that function without any such changes being detectable in the muscles related to the opposite function.

Further, it was clear that the theory advanced by Mackenzie, and which has since found favour with many, viz., that possibly the reason why the abductor fibres succumb before the adductor in affections of the nerve is because they are more superficially and circumferentially arranged, while the adductor fibres are situated deep in the substance of the nerve, is shown by these experiments to be entirely erroneous.

One point which is difficult to explain is why there should be so marked a difference between the recurrent laryngeal nerve of a young and that of an adult dog, as regards the respective predominance of abductor or adductor representation in the trunk of the nerve. Possibly the reason why the abductor influence is in the ascendency in the young dog is because the power of phonation is still imperfectly developed, and with it both the muscle and nerve fibres subserving this function are also imperfectly developed, while the function of respiration is from the beginning fully developed, and with it the muscle and nerve fibres connected with that function. That the reverse should be the case in the adult animal may well be due to the fact that phonation is perfectly developed, while respiration has become so automatic that very feeble stimuli are necessary to keep it going.

My sincere thanks are due to Professor Victor Horsley for allowing me to carry out this research in the Pathological Laboratory of University College, London, and for being so good as to verify the results which I obtained from time to time.

I wish also to express my thanks to Dr. Felix Semon for having very kindly given me access to his valuable collection of literature on the subject, and facilitating the work of writing this paper.

VI. "Interference with Icterus in Occluded Ductus Choledochus." By VAUGHAN HARLEY, M.D. Communicated by GEORGE HARLEY, M.D., F.R.S. Received March 23, 1892.

(From the Physiological Institute, Leipzig.)

In 1880, Kufferath* pointed out that when both the ducti thoracici—right and left—and the ductus choledochus are ligatured icterus does not appear. From his having only kept the dogs experimented upon alive from 1 to 2½ hours, I was induced to test the value of the statement by a series of experiments on animals kept alive for much longer periods.

The following are the results obtained, conducted under the guidance of Professor Ludwig, to whom my best thanks are due for the valuable assistance he gave me.

The health of the animals experimented upon, I found, was not disturbed by ligaturing the thoracic duct, if, after being kept fasting a few days, they were carefully fed upon a fat-free diet; and not only so, but that they might live for weeks and months and even increase in weight. Nor did the additional ligaturing of the common bile duct prove dangerous if done a few days previous to the application of the ligature to the thoracic duct, and the dogs fed on food containing only small quantities of proteids.

On the other hand, when both the thoracic and bile ducts were ligatured at the same time, the animals frequently died a few days after the operation, in consequence of rupture of the common bile duct and the escape of bile into the peritoneal cavity inducing fatal peritonitis.

The experiments were conducted as follows:—After being operated upon, each dog was kept separately in a place suitably arranged for collecting its urine, which was daily examined for bile pigment by Gmelin's test, and for bile acid (after being treated according to Hoppe Seyler's method) by von Udranszky's test. After the death of the animal the bile in the gall bladder was analysed. The portal blood-vessels were injected, and, although the bile capillaries were greatly distended, all attempts made to inject them proved unsuccessful. In order to ascertain if the thoracic duct had been properly ligatured it was carefully examined, and then injected with Berlin blue, so as to find out if it had opened up any collateral lymphatics, through which its lymph could find access to the general circulation.

Of eighteen dogs thus experimented upon, two having died from

* Kufferath, "Ueber die Abwesenheit der Gallensäure im Blute nach dem Verschluss des Gallen- und des Milchbrustganges" (Du Bois-Reymond's 'Archiv f. Physiol.,' 1880, p. 92.)

blood poisoning from three to four days after the operation, they are not included in the following tables.

Of the sixteen dogs which survived the operation—from the day after both their thoracic and bile ducts were ligatured—eight passed urine containing neither bile pigment nor bile acid. In all of these cases the thoracic and common bile ducts had been ligatured at the same time.

Results arranged according to the duration of life.

No. of experiment.	Lived.	Bile absent from the urine for	Cause of death.
1	20 days	11 days	Killed.
5	18 „	17 „	Rupture of bile duct.
2	7 „	7 „	Peritonitis.
6	7 „	6 „	Rupture of bile duct.
4	5 „	4 „	„ „ „
3*b*	4 „	4 „	„ „ „
3*a*	3 „	3 „	Peritonitis.
10	3 „	2 „	Rupture of bile duct.

In the remaining eight cases the thoracic duct was ligatured some days after the common bile duct, and in them the bile pigment and bile acid, which had appeared in the urine as soon as the bile duct was ligatured, disappeared from it on the thoracic duct being also operated upon, except in those cases where a sufficient time had been allowed to elapse after the two operations to admit of collateral lymphatics being formed.

Table of Results obtained, arranged according to the time allowed to elapse between the two operations.

No. of experiment.	Thoracic duct ligatured after the choledochus.	Urine contained bile until	Urine free from bile.	Killed.
7	On 13th day.	26th day.	..	27
14	„ 9th „	56th „	..	56
13	„ 7th „	23rd „	..	23
8	„ 6th „	14th „	From 15th to 18th day.	18
9	„ 6th „	11th „	..	11
15	„ 5th „	6th „	From 6th to 13th day, returned on the 14th day.	31
17	„ 4th „	16th „	..	16
16	„ 2nd „	7th „	From 7th to 15th day.	28

By careful post-mortem examination it was ascertained that in every case in which bile appeared in the urine, after ligaturing the thoracic duct, the bile had reached the general circulation by the development of collateral lymphatic vessels. These were found to leave the thoracic duct at a point under the first rib and proceed from thence to join the right innominate vein. The only other occasions on which bile was found in the urine, after ligature of the thoracic duct, were in those cases where rupture of the bile duct occurred, and allowed its contents to escape into the peritoneal cavity.

In every case examined, not only was bile pigment, but also bile acid, found in the lymph of the dilated thoracic duct.

Tiedemann and Gmelin,* Fleischl,† and Kunkel‡ had already pointed out that when the ductus choledochus is ligatured and the lymph collected from the thoracic duct it is rich both in bile pigment and bile acid, whereas the blood remains entirely free from bile.

The analyses of the bile taken from the gall-bladders of the dogs in which a communication between the thoracic ducts and the right innominate vein had been established by the development of collateral lymphatic vessels, thereby permitting bile to reach the general circulation, showed that, in these instances, the bile neither contained an increased quantity of soluble substances nor of taurocholate of soda, but merely an excessive amount of mucin.

The following table contains the results obtained from the analyses of bile taken from the gall bladders of four of the dogs operated upon, and of two other healthy dogs. The amount of taurocholate of soda was calculated from the quantity of sulphur found in an absolute alcohol extract of the dried residue left after the removal of mucin, and other substances soluble in ether (cholesterin, fat, and lecithin).

Analyses of Bile from Gall Bladders.

No. of experiment.	Lived.	Mucin.	Dried residue.	Taurocholate of soda.
13	23 days	3·706 p.c.	16·164 p.c.	13·359 p.c.
15	31 „	1·991 „	7·974 „	4·628 „
5	18 „	1·231 „	11·810 „	7·661 „
8	18 „	1·183 „	15·084 „	9·415 „
Normal dog	..	0·746 „	22·460 „	
„ „	..	0·691 „	11·732 „	6·620 „

In those cases where the bile did not appear in the urine for some

* Tiedemann u. Gmelin, 'Die Verdauung nach Versuchen,' vol. 2, 1827, p. 40.

† E. Fleischl, 'Arbeit. d. physiol. Anst. zu Leipzig,' 1875, p. 24.

‡ A. Kunkel, *ibid.*, 1876, p. 116.

days after ligaturing the ducts (Experiments 1, 2, 5, &c.), the bile ducts were found so greatly increased in size that on making a section of the liver, even the so-called bile capillaries could be seen by the naked eye without the aid of injection. In all cases where the blood vessels were filled with injection, and the liver hardened in a solution of bichromate of potassium, free spaces were found between the blood capillaries and the liver cells. These appeared to be the perivascular lymph spaces of MacGillavry* and Budge.† The enlargement of the perivascular lymph spaces seem to have taken place at the cost of the liver cells, for not only did the cells themselves appear to be much smaller, but the nuclei of the neighbouring hepatic cells appeared to be closer together than normal.

Three conclusions may apparently be drawn from the results obtained from these experiments :—

Firstly. That bile existing in the bile ducts can only reach the blood through the intervention of the lymphatics.

Secondly. Seeing that lymphatics surround the liver blood vessels, one is forced to believe that bile pigment and bile acid cannot pass through the endothelium of the blood capillaries in the liver; or, perhaps, even throughout the body. The fact that bile reaches the blood when it has escaped into the peritoneal cavity is no argument against this view. For in that case it would reach the blood through the lymphatics of the diaphragm.

Thirdly. After the left thoracic duct has been ligatured for some time, collateral lymphatics are opened up, or developed, leading into the right innominate vein.

VII. "On the Composition of Hæmocyanin." By A. B. Griffiths, Ph.D., F.R.S. (Edin.), F.C.S., &c. Communicated by M. Foster, Sec. R.S. Received March 16, 1892.

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* MacGillavry, "Zur Anatomie der Leber" ('Wien, Akad. Sitzber.,' vol. 50, Abth. 2, 1865, p. 207).

† Budge, "Ueber die Lymphgefässe der Leber" ('Berichte der K. Sächs. Gesell.,' 1875, p. 161.

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"The Nature of the Shoulder Girdle and Clavicular Arch in Sauropterygia." By H. G. Seeley, F.R.S., Professor of Geography, King's College, London. Received January 18,—Read February 18, 1892.

I. The Nomenclature of the Bones of the Shoulder Girdle.

§ 1. *In Ichthyosauria.*

The Sauropterygia and Ichthyosauria having formerly been combined in the group termed Nexipoda or Enaliosauria, it has been rather assumed than proved that the bones which form the shoulder girdle in those orders are homologous. The Ichthyosaurian shoulder girdle was well figured by Sir E. Home ('Phil. Trans.,' 1818, Part I) and Cuvier ('Oss. Foss.,' Pl. 258). Figures by other authors agree substantially (Huxley, 'Anatomy of Vertebrates,' p. 244) in showing (1) that the coracoids meet ventrally in the median line; (2) that there is a notch on the anterior margin of the coracoid between the median anterior cartilaginous border of the bone and the scapula, and this notch varies in depth and width with the species; (3) the scapula is directed outward, upward, and forward; (4) its articular end has a posterior part which contributes with the coracoid to form the glenoid cavity for the head of the humerus, a median part, which articulates with the anterior articular edge of the coracoid, and an anterior surface, which does not differ in its cartilaginous articular aspect or thickness from the middle portion, but which looks inward without any bony element of the shoulder girdle to articulate with it. This condition has not been explained. At one time I doubted the existence of such a surface in the undisturbed skeleton ("Pectoral Arch, &c., of *Ophthalmosaurus*," 'Geol. Soc. Quart. Journ.,' December, 1874, p. 698), and some subsequent writers have restored the shoulder girdle as though no such surface existed (J. W. Hulke, "Presidential Address, Geol. Soc., 1883," p. 19, copied by R. Lydekker, 'Cat. Foss. Rept. and Amph. Brit. Mus.,' Part II, 1889); (5) the scapula carries the rod-like clavicle upon the anterior margin of the bone, and from the posterior or ventral surface of the clavicles the median bar of the interclavicle is prolonged backward ventrally upon the coracoid bones.

Since 1874 I have examined most of the Ichthyosaurian skeletons from English and German strata without finding a specimen which

leads me to doubt the substantial accuracy of the early interpretations of Home, Buckland, and Cuvier, in regarding the scapula as extending an articular surface inward and forward towards the pre-articular portion of the coracoid. Various circumstances lead me to suggest that the notch on the anterior margin of the coracoid is a portion of the precoracoid foramen; that the precoracoid element of the shoulder girdle was cartilaginous in *Ichthyosaurus;* and that this cartilage usually articulated with the part of the scapula anterior to the external articulation of the coracoid, and also with the anterior inner processes of the coracoids, so as to complete the precoracoid foramen anteriorly. Among the reasons which suggest this interpretation are: (1) It accounts for the structure of the shoulder girdle, and explains its homology; (2) it brings the shoulder girdle of *Ichthyosaurus* into harmony with *Nothosaurus*, in which there is a similarly incomplete (precoracoid) foramen and similar cartilaginous surfaces of coracoid and scapula in close juxtaposition; (3) it brings the shoulder girdle of *Ichthyosaurus* into harmony with that of the Anomodontia, because they correspond in the form of the scapulæ, the positions and forms of the clavicles, interclavicles, and coracoids; so that, if the Anomodont precoracoid were unossified, the differences from *Ichthyosaurus* would be small, except that some of the Anomodonts (*Pareiasaurus*) develop an epiclavicle of Labyrinthodont type. On this evidence a cartilaginous precoracoid is shown in the restoration now given.

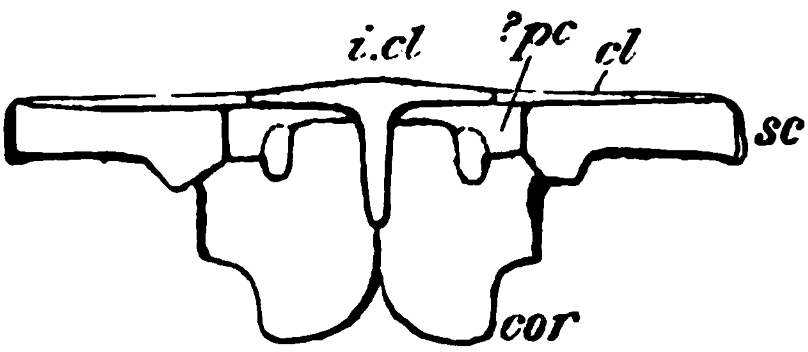


FIG. 1.—Shoulder girdle of *Ichthyosaurus*. *cor.*, coracoid; *sc*, scapula; *? pc*, precoracoid, supposed to have been cartilaginous; *cl*, clavicle; *i.cl*, interclavicle.

Before the Enaliosauria were subdivided, the bone which is here named interclavicle was regarded by Sir E. Home as homologous with the interclavicle of *Ornithorhynchus*, but it is named episternum by Cuvier, Sir R. Owen, and some recent writers like K. von Zittel. To the best of my belief the episternum was identified as being the interclavicle by Professor Huxley. My own earliest use of the term in relation to *Ichthyosaurus* is in 1869 (' Index to Fossil Remains, &c., Woodw. Museum '). If the ossifications are membrane bones, they are rightly classed as clavicles; if they are cartilage bones, they may be connected with the sternum, and take a sternal name. Each of

these three bones terminates in a sharp edge or point, and no one of them shows a terminal surface which has the aspect of being cartilaginous; there is therefore no evidence of a cartilaginous origin, while the mutual relations of the bones and their forms closely parallel the clavicles in Chelonians and some Lacertilians; they are therefore identified with those elements,* because there is no ground for regarding them as parts of the sternum.

§ 2. *In Sauropterygia.*

The nomenclature of the bones of the shoulder girdle in the Sauropterygia has given rise to greater differences of opinion.† The Amphibian mode of ossification of the long bones by long conical epiphyses‡ connected by a dice-box shaped shaft, coupled with a general resemblance of the pelvis and shoulder girdle to those of Chelonians, led to the conclusion that, while the scapula, precoracoid, and coracoid are all separate from each other in some existing Amphibians, the Amphibian plan had been modified in the Chelonia by the scapula and precoracoid remaining undivided, while in the Sauropterygia the coracoid and precoracoid remained in primitive undivided condition. The strongest argument against this is the absence of a recognisable coracoid foramen in Sauropterygia in the coracoid bone,§ which, when present, may be supposed always to occur at the junction of the coracoid and precoracoid elements. At the same time the clavicles were recognised in the Sauropterygia as distinct lateral ossifications, placed at the sides of the interclavicle, and closely united with it by suture in some species of *Plesiosaurus*.

The difficulty with the scapular arch is in the theoretical postulates concerning the elements of which its several parts consist. Sir R. Owen ('Brit. Assoc. Rep.,' 1839, p. 56) used the Chelonian hypothesis to explain the scapula in *Plesiosaurus*. And E. D. Cope ('Amer. Phil. Soc. Trans.,' 1870, p. 51), in *Elasmosaurus*, regarded the ventral plate of the bone anterior to the coracoid as clavicles or procoracoids, assuming a scapula external to it, as in Chelonians. And in 1883 Mr. Hulke (*loc. cit.*) also urged that the Plesiosaurian bone, usually named scapula, is a compound bone formed of scapula and precoracoid, as in Chelonians.

In support of this interpretation, it might be urged that the bones

* Hulke, *loc. cit.*

† 'Those which prevailed prior to 1874 were summarised in the 'Quart. Journ Geol. Soc.,' for that year.

‡ 'Index to Fossil Rept. Woodw. Mus.,' pp. 98, 120, 1869.

§ The specimen figured 'Quart. Journ. Geol. Soc.,' 1874, p. 446, is suggestive, but not conclusive, for the bone is very thin, and no portion of the margin of the perforation is seen, so that its existence as a normal character is unproved.

are similarly situate in both ordinal types, and similar in having a scapular portion which extends vertically, and a precoracoid portion which extends inward horizontally to the median line. On the other hand, there is no close resemblance between these Orders in the bones in question. (1.) The coracoids are dissimilar in form, and are differently conditioned, for they do not meet in the median line in any Chelonian, while there is no Sauropterygian in which they have not a mesial union. (2.) In Mr. Hulke's figures it is only the anterior portion of the ventral plate of the scapula which is lettered as precoracoid. Thus the precoracoid does not enter into the humeral articulation, or hold any position which theoretically can be compared with the bone in Chelonia; while the clavicular arch is anterior to the supposed precoracoid in Sauropterygians, but holds no comparable relation in Chelonians. This latter difficulty apparently led Mr. Hulke to regard the bones termed clavicular as omosternal in Sauropterygia. But no Chelonian possesses an omosternum; so that, if the identification were demonstrated, it does not support the Chelonian hypothesis of the shoulder girdle.

First, it may be observed that it is only in Anura that the precoracoid enters the anterior margin of the glenoid cavity; but in Urodela the precoracoid appears to be excluded, so that it is not theoretically impossible on an Amphibian hypothesis for the precoracoid to be anterior to the acetabulum; but the bone is always wedged between the scapula and coracoid, and on the coracoid border the coracoid foramen is always persistent, so that there is no analogy between Urodele and Sauropterygian to sustain the identification of the precoracoid which has been offered. Whenever two divergent bones form the scapular arch those two bones are the coracoid and scapula; but there is no analogy to support the hypothesis that the precoracoid might form the free extremity of the scapula, as in Mr. Hulke's figure (*loc. cit.*). There is no conclusive evidence of the mutual relations of the scapulo-precoracoid to the glenoid cavity in the Chelonia, but, unless it could be shown that the relations of these bones to the shoulder girdle were the same in both types, Chelonian analogy with Sauropterygia in this part of the skeleton rests upon an inconclusive basis of fact. In Chelonians the ascending process of the scapula extends dorsally towards the vertebræ, while in Sauropterygia it extends backward above the glenoid articulation for the humerus, and there is no evidence that these structures are homologous.

If the evidence is insufficient to sustain the interpretation discussed, it is found that the precoracoid has disappeared as a separate element from the skeleton in Lacertilia, and in most existing Ornithomorpha ('Roy. Soc. Proc.,' vol. 49, p. 520). It is recognised in association with the coracoid in certain Birds; and the persistence of the coracoid foramen gives some evidence that the precoracoid is not unrepre-

sented on the scapular side of the foramen in all members of that class except the Ornithosauria. Its individuality is retained in some of the Sauromorpha; and, although they have no distinct osseous representative of the bone, the nearest analogy to the shoulder girdle in Sauropterygia is found among Nothosauria; and there is no doubt that these resemblances and those with Anomodonts are closer than with existing orders of animals.

The Nothosaurian shoulder girdle contains the same number of constituent elements as in Sauropterygians, and the same nomenclature has been applied to them. There are some slight differences in the coracoid. In the Nothosauria it lies more obviously behind the glenoid cavity, while in many Plesiosaurs, especially the typical forms from the Lias, it also has a considerable median anterior extension. Further, in Nothosaurs there is a notch in the anterior margin of the coracoid, already contrasted with the similar notch in the coracoid of Ichthyosaurs, anterior to which are rough cartilaginous surfaces of scapula and coracoid, which have the aspect of having supported a cartilage which completed the coracoid foramen. There is no anterior prolongation of the scapula in *Nothosaurus* such as is seen in *Plesiosaurus*, but the clavicles are much elongated. They form a squamous overlap on the visceral surface of the scapula, according to von Meyer, and their length, and prolongation forward, removes the interclavicle from contact with the coracoids. If the suggested precoracoid cartilage in *Nothosaurus* existed, it makes the nature of both coracoid and scapula clearer in *Plesiosaurus*, and shows that the precoracoid need not be displaced into the position here assigned to the clavicle.

First, the foramen which appears to be indicated in the anterior margin of the coracoid in some species of *Ichthyosaurus* as a deep narrow notch, in other species widens to a concave anterior border to the bone; and similarly, in the specimen figured by Deecke as *Lariosaurus Balsami*, there is no trace of the anterior notch in the coracoid such as characterises *Nothosaurus*, but that bone has a smooth sharp anterior concave border such as the bone shows in *Plesiosaurus*. It would therefore seem to follow that the precoracoid foramen of *Nothosaurus* becomes the coraco-scapular foramen of *Plesiosaurus*, and that the precoracoid in Elasmosaurs ceases to exist as a distinct cartilage. It cannot be inferred to be lost by connation with the coracoid, because the foramen might then be supposed to persist, but, as there is no foramen in either the scapula or coracoid,* there is no evidence of the composite nature of either bone in *Plesiosaurus*. Nevertheless, since the precoracoids meet in the median line in many Amphibians, and in Chelonians, and the scapulæ never have a median ventral union, there is an *a priori* probability that bones formed from cartilage, placed

* Always subject to the doubtful evidence of the Brit. Mus. fossil 2041*.

anterior to the coracoids, meeting in the median line, should rather be precoracoids than scapulæ in such Sauropterygia as show these characters. It has already been shown to be probable that the foramen anterior to the coracoid is the precoracoid foramen, having undergone such an enlargement in transition from *Nothosaurus* to *Plesiosaurus* as does the obturator foramen between the pubis and ischium in transition from the pelvis of *Dicynodon* to the pelvis of a Mammal. Therefore the precoracoids may have ceased to be differentiated, even as separate cartilages, and the coracoids may have grown forward at the expense of this cartilage, just as the scapulæ extended inward and backward at its expense; so that, while the scapulæ are conveniently so named, it may be recognised that in *Elasmosaurus*, *Colymbosaurus*, *Murænosaurus*, and their allies, the parts of the bone which meet in the median line, and are in median contact with the clavicular arch, are theoretically in the position of precoracoid elements, which connect the scapulæ with the coracoids. But since the Plesiosauridæ show no such median union of scapular elements, or ossifications in front of the coracoids, it follows that there is no evidence that the precoracoid was ossified at all, while the cartilage representing it, if present, must have been a slender bar, comparable to the suggested precoracoid cartilage in *Ichthyosaurus*, as shown by the absence of a thick cartilaginous truncation of the anterior median termination of the coracoids in *Plesiosaurus*.

In the Anomodontia the plan of development of the shoulder girdle has been modified by the great extension of the clavicular arch outward and upward, so that the scapulæ are rather on the type of the Ichthyosauria than of the Sauropterygia. But the position and relations of the Anomodont precoracoid furnish some support to the interpretation given to the element in Ichthyosauria and Sauropterygia; because, if the precoracoid foramen in Anomodonts were theoretically enlarged to the dimensions seen in *Colymbosaurus*, *Plesiosaurus*, or *Lariosaurus*, it would be manifest that for so long as it connected the scapula and coracoid it was Elasmosaurian; so long as it remained attached to the extremity of the scapula only it would be Plesiosaurian; and so long as a remnant remained of cartilage in contact with the inner border of the clavicle the condition would be Lariosaurian.

There is thus a fundamental difference of plan between the imperforate coracoid of Sauromorpha and the perforate coracoid of Ornithomorpha, which depends upon the way in which the precoracoid bone loses its individuality.

§ 3. *Nomenclature of the Bones in the Clavicular Arch.*

Early writers regarded the median bone anterior to the coracoids in *Plesiosaurus* as the sternum. Sir R. Owen named it episternum. Pro-

fessor Huxley regarded it as interclavicle and clavicles ('Anatomy of Vertebrated Animals,' 1874, p. 210). In 1874 I figured the clavicles as posterior to the interclavicle in *Plesiosaurus Hawkinsi*, and drew attention to the similar condition in *Pl. laticeps* ('Geol. Soc. Quart. Journ.,' 1874, p. 444, since figured by Zittel). Mr. Hulke, in 1883 ("Presidential Address, Geol. Soc.," p. 20), regards these ossifications as indivisible, and names the mass omosternum, thus reverting to the hypothesis that the ossifications have a cartilaginous origin, and are episternal. It follows from Mr. Hulke's views that the reputed clavicles of *Nothosaurus* are precoracoid, and the median bone between them is the omosternum.

The late Professor W. K. Parker fully discussed the omosternum in the Vertebrata. It is found in Mammals and in Anura, but is not present in all Anura, and is not always ossified. In the genus *Calamites* it appears to extend slightly on the visceral surface of the precoracoids. In the Amphibian group which it characterises clavicles are probably not found, so that it is in place of an interclavicle, if it does not represent it. It is sometimes single, sometimes paired, but never tripartite, as the median bone among Sauropterygians. Among Mammals Mr. Parker found the omosternum (paired) uniting with the sternum, while laterally it is continued by the clavicles, though there is a pair of small cartilages, termed precoracoids, between it and those elements of the skeleton. In the Monotremata the interclavicle is in the position of the omosternum. In *Anguis fragilis* Mr. W. K. Parker figures both interclavicle and clavicles, but there is no omosternum. The omosternum behaves as though it were the name given to the interclavicle when that element ossifies from cartilage.

A sternum is developed in every existing animal in which the omosternum is present, but in no Sauropterygian is there ever any trace of a sternum, so that there is nothing to suggest an omosternum. The omosternum has not been recognised in any existing order of Reptiles, and the Sauropterygia is the only fossil type except the Nothosauria in which it has been supposed to be found. That suggestion appears to rest upon the fact that the omosternum is found anterior to the precoracoids in certain existing Amphibia. There is the circumstance that the bones in *Plesiosaurus* extend on the visceral surfaces of the scapulæ and coracoids, while the clavicles in *Ichthyosaurus* are on the anterior and ventral surfaces of the same bones; but no animal is known in which the omosternum is developed in the position of the bone which has been so named in *Plesiosaurus*, and, so far as position goes, there is no evidence known to me which suggests that the bones in question should be omosternal rather than clavicular.

The omosternum has never been shown to consist of a T- or V-

shaped median piece flanked by separate lateral ossifications as in *Plesiosaurus*, while this condition parallels the interclavicle and clavicles in all animals in which they are found.

It has never been shown that any one of the bones in question in *Plesiosaurus* retains a surface which has the aspect of having been cartilaginous. On the contrary, every specimen which I have examined is more or less thin and squamous, with contours completely ossified to sharp edges, even in the most immature specimens; while the interclavicle, when preserved, unites with the clavicles either by a thin squamous overlap or by sagittal sutures. This condition seems to me to demonstrate that the bones are membrane bones. I submit it follows that they are clavicles, and therefore that the visceral position of the clavicular arch, although anomalous, is not inconsistent with clavicular homology. Bone for bone, the three clavicles in *Plesiosaurus* seem to me to correspond to those of *Ichthyosaurus* and *Nothosaurus*. In the former their union is usually squamous, in the latter it is sutural. In Sauropterygia both conditions are found. The proposal made to identify the three anterior bones in the shoulder girdle in *Nothosaurus* as omosternum and precoracoids introduces the precoracoid as a distinct bone,* which is not known to be paralleled in any allied group of animals except the Anodomontia, in which there is no omosternum, and where the precoracoids are differently conditioned, being in the closest union with the coracoids, with a well-developed clavicular arch. But when the supposed precoracoids of *Nothosaurus* are recognised as clavicles, which rest by squamous overlap on the visceral surfaces of the scapulæ, like the clavicles of *Plesiosaurus*, the clavicular arch is in harmony with that of the Sauropterygia, and the supposed differences in its composition disappear.

There are two family types in the Sauropterygia defined by differences in the shoulder girdle and other characters, known as Plesiosauridæ and Elasmosauridæ, though the organic differences which characterise them have not been fully set forth.

II. Further Evidence of the Nature of the Clavicular Arch in the Plesiosauridæ.

§ 1. *Nature and Limits of the Family.*

There are four principal genera of Plesiosauridæ, which are named *Plesiosaurus*, *Eretmosaurus*, *Rhomaleosaurus*, and *Pliosaurus*. The family is characterised by the cervical ribs being attached to the vertebræ by more or less completely-defined double facets and by the scapulæ being separated in the median line by the clavicular arch, by

* Hulke, *loc. cit.*

which they are braced to the coracoids. In the British Museum Catalogue ('Fossil Rept. and Amph.,' Part II), the Plesiosauridæ is made to also include the Elasmosauridæ, and the genera are enumerated in the following order:—*Pliosaurus*, *Peloneustes*, *Thaumatosaurus*, *Polyptychodon*, *Cimoliosaurus*, *Eretmosaurus*, *Plesiosaurus*. I should restrict the family to the fossils indicated by the names *Pliosaurus*, *Peloneustes*, *Thaumatosaurus*, *Eretmosaurus*, and *Plesiosaurus*. Good skeletons of these genera are known with the exception of *Thaumatosaurus*, which was founded by von Meyer ('Palaeontographica,' vol. 6) upon remains which closely resemble those of *Pliosaurus*. And, after examining the type specimens, which are imperfect cervical vertebræ, dorsal vertebræ, teeth, and portions of the hinder region of the maxillary bone, I was unable to discover any character inconsistent with reference of the species to *Pliosaurus*. The head was evidently as large as in *Pliosaurus;* the teeth are circular in the crown, and show no trace of the area more or less flattened and free from carination defined by a lateral ridge on each side which characterises the anterior teeth of *Pliosaurus grandis*, resembling in this respect the posterior teeth. In the late cervical vertebra figured by von Meyer, the centrum has the same form and relative shortness from front to back as in *Pliosaurus;* the articular facet for the rib is similarly elevated, has a like transverse division forming a superior subtriangular part and an inferior transversely ovate part. The only characters in which there is not absolute agreement with the English species are that the articular faces of the centrums are more circular and more concave. These differences may be of specific value; and von Meyer's species may be classed as *Pliosaurus oolithicus*, till it is fully known. For similar reasons I am unable to separate *Peloneustes* from *Pliosaurus*. And if the type species was originally referred to *Plesiosaurus*,* it was because I then regarded the subtriangular crowns of anterior teeth in *Pliosaurus* as a generic character, and that character now seems less important. It has been necessary thus to explain differences of nomenclature, because the genus *Thaumatosaurus* ('Brit. Mus. Cat. Foss. Rept.,' Part II) has been made to include six species in addition to the type, which, with one exception, are all from the Lias. They were previously named *Rhomaleosaurus Cramptoni*, *Plesiosaurus arcuatus*, *P. megacephalus*, *P. carinatus*, *P. propinquus*, *P. indicus*. I am unable to place any of these species in *Pliosaurus* or *Thaumatosaurus*, nor is there evidence that all are referable to one genus; and it does not appear that a genus based on characters drawn from this assemblage of species can displace the definite conception of von Meyer indicated in the type of *Thaumatosaurus*. Most of these species not included in *Rhomaleosaurus* appear to belong to *Eretmosaurus*.

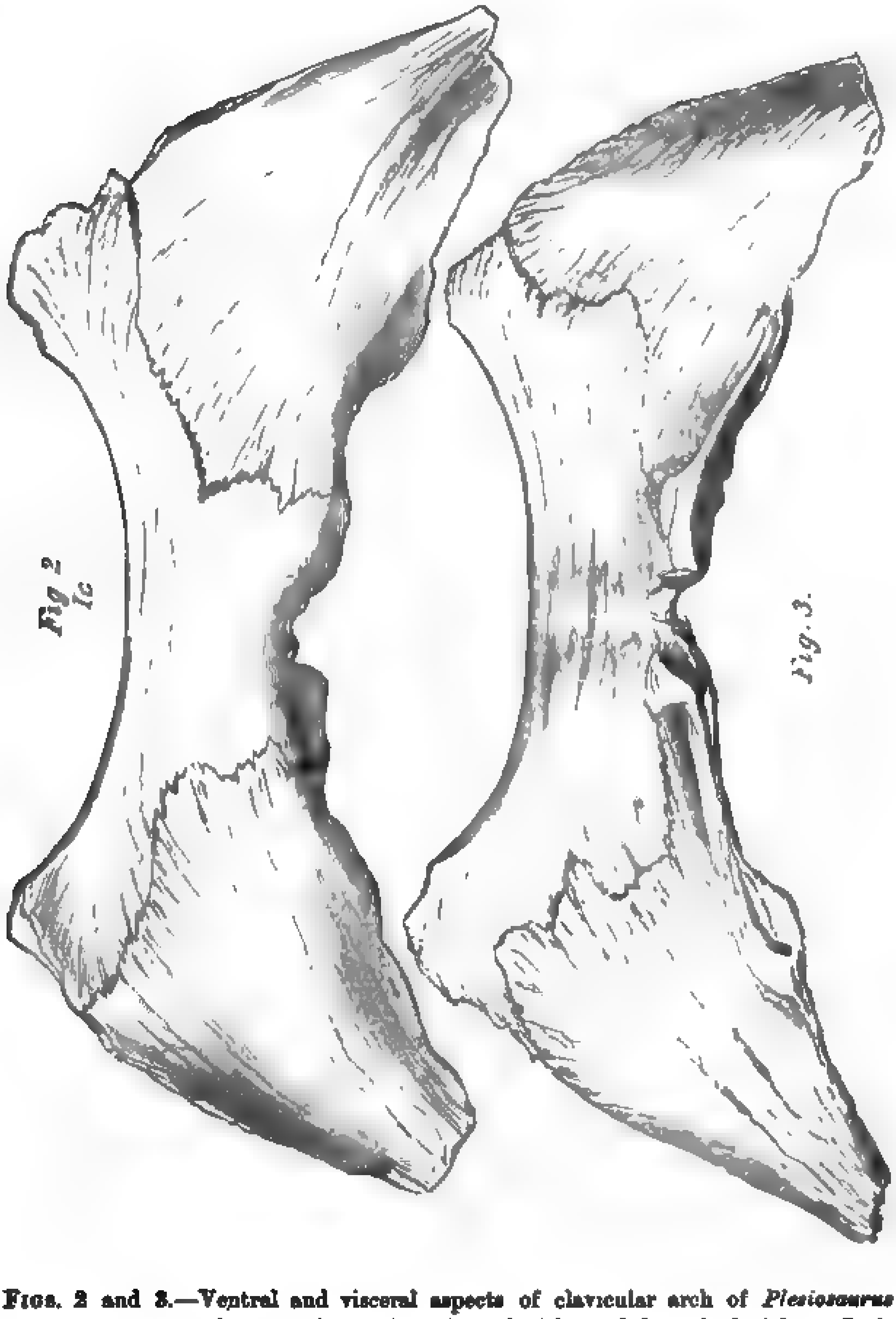
* 'Index to Aves, Ornith., and Rept. in Woodw. Mus.,' 1869, p. 139.

§ 2. *The Clavicular Arch.*

(i.) Since the clavicular arch was figured in *Pl. Hawkinsi* ('Geol. Soc. Quart. Journ.,' 1874, p. 444), v. Zittel has figured the clavicular bones in *Pl. laticeps* ('Handbuch der Paläontologie,' vol. 3, p. 489); but, while the clavicles are clearly shown, the interclavicle is named episternum. The most important evidence of this structure in Plesiosauridæ, however, is to be seen in *Plesiosaurus arcuatus* ('Brit. Assoc. Rep.,' 1839, p. 76; and 'Cat. Foss. Rept. and Amph.,' Part II, p. 163), preserved in the British Museum. From that specimen, No. 2028*, the character has been attributed to *Thaumatosaurus* (*loc. cit.*, p. 159): "Omosternum consisting of a large single plate, much expanded transversely, with a wide and shallow anterior notch."† The anterior margin of the interclavicle in this specimen resembles in contour that attributed to *Eretmosaurus* ('Geol. Soc. Quart. Journ.,' 1874, p. 445) in its wide open curvature; but there is no evidence to show whether the shoulder girdle, pelvis, and limbs in *Plesiosaurus arcuatus* were constructed on the same plan as in *Pl. rugosus*. There is no doubt that the bone consists of three distinct elements united by sutures. These are a median interclavicle and two lateral bones which I regard as clavicles. On the visceral aspect the triangular clavicles are separated from each other by the wide short posterior median bar of the interclavicle, but the clavicles extend forward so that only a narrow transverse bar of the T-shaped interclavicle is exposed in front of them, extending across the entire width of the bone. The interclavicle is $10\frac{1}{4}$ inches wide, concave on its anterior margin, $1\frac{1}{2}$ inch from front to back at the widened extremities of the cross-bar, and $\frac{8}{10}$ inch in the same measurement towards the oblong middle portion of the bone. The right anterior transverse limb of the cross-bar is 4 inches wide; the left limb is 3 inches wide. The middle portion of the bone is $3\frac{1}{4}$ inches wide and $2\frac{1}{2}$ inches in antero-posterior measurement. The sutural line which defines the interclavicle is sagittal, and consequently irregular. On each side of this T-shaped interclavicle (fig. 2), in contact with the posterior margin of its transverse bar and the lateral margin of its short wide median stem, is a large triangular clavicle which is directed backward and outward. In harmony with the dimensions of the transverse bar, the right clavicle is the wider. Anteriorly it is $4\frac{3}{4}$ inches wide; it is nearly 6 inches long. The external border, which is slightly convex, is continuous with the truncated lateral termination of the interclavicle in front of it. These external margins diverge outward as they extend backward, so that the transverse measurement over the posterior extremities of the clavicles is $14\frac{3}{4}$ inches. The postero-internal

† Compare Sollas, 'Geol. Soc. Quart. Journ.,' vol. 37, 1881, p. 457.

contours of the clavicle are irregularly concave, and as they extend inward are continuous with the posterior border of the interclavicle,



FIGS. 2 and 3.—Ventral and visceral aspects of clavicular arch of *Plesiosaurus arcuatus*, showing the median interclavicle and lateral clavicles. *Ic* is placed on the anterior margin.

and as they extend outward approximate toward the external contour of the bone without meeting it posteriorly in a point.

The ventral aspect of the clavicular arch is different (fig. 3) owing to variation in the positions of the sutures between the bones. The interclavicle no longer shows the T-shaped contour of the visceral surface, but is a wide curved bar with an irregular sagittal termination on its postero-lateral extremities. This is owing to the method of its squamous interlocking with the clavicles, which overlap its visceral surface more in front, and overlap its ventral surface more behind, where their pointed extremities nearly meet each other in the median line behind the interclavicle, and in the inch of space from which they are absent there is a slight distortion of the bone, and some evidence of a median posterior notch. The triangular forms of the clavicles are more marked on this aspect of the bones than on the other.

The most remarkable character here shown is the squamous sutural interlocking of the three bones by which their shares in forming the clavicular arch is definitely established. It is also shown by different directions of the lines of growth in the clavicles and interclavicles.

An isolated clavicular arch in the British Museum, R. 1322, presents a similar character and form, and shows in its sutures similar evidence of composite character. It has been assigned to the species named *Plesiosaurus megacephalus* (Stutchbury) in the British Museum Catalogue. It has a similar resemblance to the anterior contour of the interclavicle in *Eretmosaurus*, but I am aware of no evidence by which the species is identified from this bone, beyond a general resemblance to some specimens in the Bristol Museum.

The correspondence of structure in these clavicular arches with that figured in *Plesiosaurus Hawkinsi* and *Plesiosaurus laticeps* is a coincidence of plan, though the difference may indicate a sub-genus, and shows, I submit, that the original definition of the bones was not a conjectural suggestion, as stated by Professor Sollas, but a recognition of sutures which separate the interclavicle from the clavicles. And it seems to me a sound induction that whenever the margins of the clavicular arch are concave in front and behind, those concavities border the interclavicle, and whenever there are wings produced outward and backward, as in the specimen now figured, those wings are formed by the clavicles in all Plesiosauridæ.

(ii.) Sir R. Owen, in 1841 ('Brit. Assoc. Rep.,' p. 64), remarks on the shoulder girdle of *Pliosaurus*:—"The pectoral arch owes its chief strength to a pair of immensely expanded coracoids, having a broad and short entosternal bone on their anterior interspace, and supporting the clavicles or acromion productions of the scapulæ."* Subse-

* I have examined the specimens in the Museum of the University of Oxford

quently ('Geol. Soc. Quart. Journ.,' 1883, p. 135) a diagram of the shoulder girdle in this genus was given by that author, which represents the scapula and coracoid as meeting each other on the Elasmosaurian plan; but, unlike Elasmosaurians, the scapulæ are divided from each other on the visceral aspect by a long triangular interclavicle (named episternum) which shows a mesial notch in front. I have not seen this specimen, which is not assigned to any species, locality, or collection. It would appear to show an intermediate condition between Plesiosaurs and Elasmosaurs, but it is impossible for me at present to affirm this. No specimen is known to me which shows that in *Pliosaurus* the scapula and coracoid completely enclose the coracoid foramina. The evidence is imperfect, but it leads to the conclusion that the shoulder girdle was Plesiosaurian in plau.

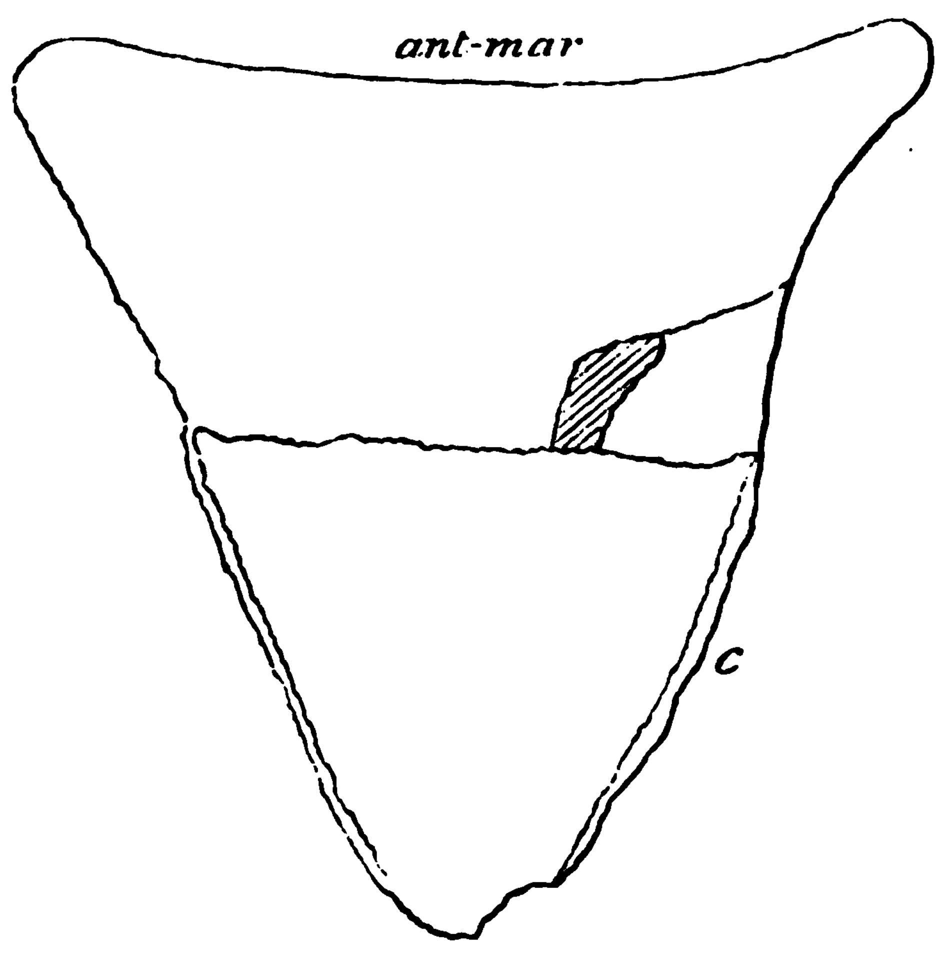


FIG. 4.—Interclavicle, *Pliosaurus philarchus*. *ant-mar*, anterior border; *c*, a lateral surface which may have been a clavicular attachment.

Pliosaurus philarchus, on which the genus *Peloneustes* has been founded ('Cat. Foss. Rept. and Amph.,' Part II), in form of the scapula closely resembles Pliosaurian remains in the British Museum. Their approximating margins are convex, and between those margins Mr. Lydekker has inserted the interclavicle (termed omosternum),

with Professor A. H. Green, F.R.S., without finding evidence of this entosternal part of the skeletou. What appear to be scapulæ of *Pliosaurus brachydeirus* have the inner and outer borders of the bones sub-parallel, with the anterior extremity but slightly widened. Zittel has interchanged the names to Owen's figures of the shoulder girdles of *Pliosaurus* and *Plesiosaurus*. I have not seen the originals of those figures.

which is triangular, flat, very thin, and has perfectly straight sides, which, in their hinder approximating two-thirds, are slighly bevelled. There is no evidence given that the bone occupied the position which has been figured, and I see no reason for believing that it was not placed, as in other Sauropterygians, on the visceral surface of the slightly inclined scapulæ, where there is a doubtful indication of what may be an imperfectly preserved right clavicle. If the straight lateral border of the interclavicle was in contact with the flat visceral surface of the scapula, the bones would be in harmonious relation. The bevelled margin appears to look inward, and is therefore inferred to have given attachment to a lateral ossification which was still more delicately thin. This condition is shown in the following figure of the bone.

(iii.) A third modification of the Plesiosaurian type may be* indicated by the specimen in the Leeds Collection in the British Museum numbered 36. It is small, and the bones are not sharply ossified and immature, as Mr. Leeds has always believed. But I have not observed any specimen in his collection which would, with certainty, represent its adult state. The bones of the shoulder girdle are thick, and the scapula and coracoid are formed on the Plesiosaurian type, in that the inner border of the scapula gives no evidence of a median precoracoid prolongation backward to meet the coracoid. There is no indication that the coracoids and scapulæ ever met in the median line, even in the supposed adult condition, since there is no anterior median process to the coracoid; but there is a cartilaginous interval between them in front like that attributed to *Pliosaurus*. The scapula is a stout triradiate bone with a wide external process, and in form it resembles the bones attributed to *Pliosaurus*. But the cervical vertebræ have no trace of the Pliosaurian modification, and have the aspect of the vertebræ of *Plesiosaurus*, except that the articulation for the rib is not divided in the cervical region. Some Plesiosaurs from the Lias have shown the closest possible approximation of those surfaces, but the divided condition of the rib facet did not terminate with the Lias species, since some specimens from the Wealden (which are referred to *Cimoliosaurus*, 'Brit. Mus. Cat. Foss. Rept.,' Part II, p. 227, No. 2,444, No. 26,000) retain the character in a condition similar to that attributed to *Thaumatosaurus carinatus* (*loc. cit.*, p. 168, fig. 57). It may be that the imperfect ossification causes the facet of bone to appear single in this Oxford Clay fossil, while its cartilaginous terminations during life may have been divided; but so far as the evidence goes it rather suggests a sub-generic modification of the genus *Plesiosaurus* as indicated by the scapular arch, distinguished by undivided articular heads to the cervical ribs, if the adult preserved

* I am not sure that this immature Plesiosaurian type did not, on attaining maturity, become the Elasmosaurian genus *Cryptoclidus*.

the characters of the young animal. This inference is supported by the evidence of the clavicular arch, and by the large size of the radius and tibia as compared with the small size of the ulna and fibula. These bones are not in natural association, being free from matrix; but I see no reason to doubt that Mr. Leeds has arranged them in positions which are correct. The characters of the skeleton lead to the conclusion that the species is new, and could not become transformed by growth and perfected ossification into any other known species.

The following are measurements which help to define the species:—Lower jaw, 9 inches. Vertebral column, as preserved and arranged, 64 inches. Thirty cervical vertebræ, 23 inches; two pectoral vertebræ supporting ribs on the neural arch and centrum, $1\frac{3}{4}$ inch. Twenty-two dorsal vertebræ measure 22 inches; three vertebræ in the sacral region, which support ribs, partly on the neural arch and partly on the centrum, $2\frac{3}{4}$ inches. Twenty-two caudal vertebræ measure 15 inches, but the extremity of the tail is not preserved. The height of the dorsal vetebræ and neural arch is about $2\frac{3}{4}$ inches. The transverse measurement over the transverse processes of the dorsal vertebræ is $4\frac{1}{4}$ inches. The longest dorsal ribs measure about 9 inches. The ilium is 4 inches long. The transverse width over the pelvic articulation is $7\frac{1}{2}$ inches. The antero-posterior extent of the pelvis is 9 inches. The pubis measures $4\frac{1}{2}$ inches from front to back. The ischium is $3\frac{1}{2}$ inches in the same measurement toward the median line. The pelvic foramina were separated from each other by cartilage. The femur is 8 inches long and 4 inches wide. The coracoid is 7 inches long by $4\frac{1}{4}$ inches wide; the scapula is $4\frac{1}{4}$ inches in length and width. These shoulder girdle bones are exceptionally thick. The transverse width over the two clavicles is $7\frac{1}{4}$ inches.

The clavicles are thin triangular bones, perfectly ossified, with sharp well-defined margins and no signs of immaturity, probably because they are membrane bones. If they met each other in the

Fig. 5.—Clavicles of a young individual *Plesiosaurus durobrivensis*.

median line it can only have been by squamous approximation. Thus arranged they would be inclined to each other. As preserved, each clavicle is about 4 inches wide; and on its inner border measures 2¾ inches from front to back, and at the external angle the corresponding measurement is ⅝ of an inch. The anterior border is straight; the inner border is sinuous and unsymmetrical on the opposite sides; the posterior border is 3½ inches long and concave, with the concavity broken on the inner third by a sharp prominence which separates a slight inner concavity from the longer external concavity.

The external extremities of the bones are truncated and striated.

The only specimen which distantly approximates to this in the large size of the radius as compared with the small ulna is an Elasmosaurian indicated in the Leeds Collection by the number 31. In that also there is no trace of an interclavicle, but the shoulder girdle is not perfectly preserved, and its clavicles are of dissimilar form. If the scapulæ in mature individuals of this species united in the median line and extended back to the coracoids, then the fossil would be Elasmosaurian, and possibly a species of *Cryptoclidus*.

III. The Clavicular Arch in the Elasmosauridæ.

§ 1. *The Nature and Limits of the Family.*

When the Elasmosauridæ was defined in 1874 its clavicular arch was unknown and supposed to be wanting, and the family was based upon the circumstance that the bones named scapulæ met each other in the median line, and were prolonged backward to unite with the median processes of the coracoids in *Elasmosaurus* and *Colymbosaurus*. I owe a knowledge of the clavicular arch in this family to A. N. Leeds, Esq., of Eyebury, who for twenty years has collected the fossil Vertebrata from the Oxford Clay near Peterborough. In this family the cervical vertebræ have the ribs attached by undivided articular heads. The carpal and tarsal bones are polygonal and well ossified. The genera on which the family is based are *Elasmosaurus*, *Colymbosaurus*, and *Muraenosaurus* ('Geol. Soc. Quart. Journ.,' 1874, p. 436), none of which appear in the 'British Museum Catalogue of Fossil Reptiles,' Part II. The only genera in that enumeration which could be so referred are *Polyptychodon* and *Cimoliosaurus*. Excepting *Polyptychodon* only, all English as well as all American Elasmosaurians have been referred to the latter genus in the Catalogue referred to. Hence, as it will be presently shown that the Elasmosauridæ develop remarkable modifications of the clavicular arch, which may be regarded as of generic importance, it is convenient to determine as far as possible the synonymy of the genera comprised in the family.

The genus *Cimoliosaurus* figured by the late Dr. Leidy in 1865 ('Smithsonian Contributions to Knowledge') rests upon thirteen centrums of vertebræ without arches or processes, noticeable chiefly for their transverse width. Fourteen other vertebræ from the Greensand of New Jersey are described; but there is no evidence of any other part of the skeleton. Leidy expressed doubt whether his genus *Discosaurus* might not prove to be founded on vertebræ of *Cimoliosaurus*. This view was adopted by Professor Cope ('Amer. Phil. Soc. Trans.,' vol. 14), but that identification only contributed a knowledge of the carpal and metacarpal bones. Hence the characters by which the genus is defined in the 'British Museum Catalogue' are not drawn from Leidy's type.

The generic characters of *Cimoliosaurus* which may be obtained from Leidy's figures are: Articular face of the centrum flat or flattened, short from front to back, transversely extended in the cervical region. The neural arch is small, with compressed lamellar neurapophyses, which appear to be anchylosed to the centrum. The facet for the cervical rib is single, at first compressed from above downward, afterwards becoming ovate; the facets are on short pedicles. The chevron articulations impress both the anterior and posterior margins of the short centrums in the middle of the caudal series. The carpals are transversely oblong. The metacarpals and phalanges are compressed from above downward.

The name *Brimosaurus* (Leidy, 'Philadelphia Acad. Nat. Sci. Proc.,' 1854, Pl. 2, p. 72), was proposed for Plesiosaurian vertebræ which have the ventral surface flat instead of concave, as in *Cimoliosaurus;* but, as the genus is not mentioned in that author's 'Cretaceous Reptiles of the United States,' 1865, it may be regarded as probably abandoned and included in *Cimoliosaurus*.

Dr. Leidy also proposed a genus *Oligosimus* ('Philad. Acad. Nat. Sci. Proc.,' 1872, p. 39). It is unfigured and based upon an early caudal vertebra. It has the neural arch anchylosed to the centrum. A groove defines the limit of the articular face of the centrum. The chevron facets only impress the posterior border of the centrum. Its measurements are: length, 1 inch; width, 2·3 inches; depth, 1·9 inch. These characters seem insufficient at present to distinguish the type as a genus.

Professor E. D. Cope has decribed five other genera which he regards as distinct from *Cimoliosaurus;* they are named *Elasmosaurus*, *Polycotylus*, *Orophosaurus*, *Uronautes*, and *Piptomerus*.

Polycotylus from Cretaceous Limestone, near Fort Wallace, Kansas ('Amer. Phil. Soc. Trans.,' vol. 14, Part 1, p. 35, Pl. 1, 1870), is founded upon dorsal and caudal vertebræ. It is characterised by the very short dorsal vertebral centra, which are deeply biconcave. The tibia is broader than long. The neural arch is anchylosed to the

centrum, as are the caudal ribs. To these characters may be added from Professor Cope's figures; neural arch depressed, with massive neurapophyses, and small neural canal. These characters help to define the genus from *Uronautes*. Phalanges remarkably short. The author subsequently states ('Amer. Naturalist,' 1887, p. 564) that in *Polycotylus* the neurapophyses and all diapophyses and parapophyses are co-ossified with the centra.

In *Piptomerus* ('Amer. Naturalist,' 1887) the neurapophyses and all other processes of the vertebræ articulate freely with the centra. The cervical vertebræ are short, twice as wide as long, and deeper than long. The dorsal vertebræ are two-thirds as long as the cervicals, deeper, and rather narrower.

In *Orophosaurus* the neural arches are co-ossified, and the parapophyses free. The centrum is a little wider than deep.

In *Uronautes* both neural arches and parapophyses are co-ossified. All vertebræ are short, nearly twice as wide as long, as deep as wide, centrum biconcave, neurapophyses lamellar, neural canal large.

In the American specimens referred to *Plesiosaurus* Professor Cope states that the neural arches of the vertebræ are loosely articulated.

Until the American types are fully figured it will not be possible to judge whether these genera are all founded on characters which will enable them to be recognised in adult individuals.

In *Elasmosaurus* the characters given for the genus are: Neural arch anchylosed with the centrum; cervical centrum longer than deep, deeper than wide; ribs articulated to oval pits. Vertebræ numerous. The dorsal vertebræ have strong transverse processes. In the caudal vertebræ the articular chevron facets are said to be on the inferior face, near its posterior articular aspect. This condition is not unknown in early caudal vertebræ in English Sauropterygians from the Pelolithic strata, but no evidence has been given that it extends throughout the caudal series in any Sauropterygian species. The scapular arch has the well-known form, with the scapulæ meeting in the median line, and continuous posteriorly with the coracoids, so as to enclose two large foramina between the bones. The scapulo-precoracoid appears to form about two-thirds of the wall of the glenoid cavity. No clavicle was found. The ilium appears to articulate with the pubis only. No limb bone was found, nor any abdominal ribs.

Professor Cope states that this genus is distinguished from *Cimoliosaurus* by the shortness of the neck in the latter, and its elongation in *Elasmosaurus*. In *Elasmosaurus* the cervical centrum is transversely compressed, and comparatively long; while in *Cimoliosaurus* it is short, broad, and vertically depressed.

Finally, Mr. F. W. Cragin has described *Trinacromerum* ('Amer. Geol.,' vol. 2, p. 405, 1888, and vol. 7, September, 1891, p. 171) from the Cretaceous rocks of Kansas, but no figures of it have yet been

given. In it the ilium articulates with the ischium only, as in some species of *Murænosaurus* from the English Oxford Clay (Leeds Collection, Brit. Mus.). The shoulder girdle is on the Elasmosaurian plan, enclosing two vacuities, but the structure of the glenoid cavity is distinctive. There are three bones in linear succession at the distal end of the humerus and femur. The tibia and fibula are transversely extended, and of oblong form, apparently resembling *Colymbosaurus*. The phalanges are unusually numerous. The neural arch is anchylosed to the centrum. The neural canal is large. The cervical vertebræ are sub-quadrate, depressed, and transversely wide. The dorsal centrum is sub-circular. The articular faces are shallow concavities.

The characters assigned to *Polycotylus*, *Cimoliosaurus*, *Elasmosaurus*, and *Trinacromerum* are such as enable the types to be recognised; and, therefore, pending fuller information, it is convenient to adopt them as genera limited, so far as is at present known, to the Cretaceous period. It is probable that all belong to the Elasmosauridæ, but *Elasmosaurus* and *Trinacromerum* are the only types in which the shoulder girdle is known. The oblong form of the tibia in *Trinacromerum* and *Polycotylus*, and the transverse elongation of a carpal in *Cimoliosaurus*, make it probable that the middle segments of the limbs had the bones transversely elongated in all these genera. In none of them have clavicles as yet been recognised.

Polyptychodon is probably to be included with these genera; but it is only known from teeth, cranial fragments, and vertebral centra, which do not differentiate the genus; though the cervical vertebræ ('Quart. Journ. Geol. Soc.,' vol. 32, p. 433) are relatively short and deep.

The Elasmosauridæ are well represented in the Cretaceous rocks of this country. Two genera, *Murænosaurus* and *Colymbosaurus*, have also been regarded as peculiar to the Oxford and Kimeridge Clays. These genera are best defined by the bones of the extremities. In both the bones of the shoulder girdle are essentially the same.

In *Murænosaurus* the cervical region is long. The zygapophysial facets have a cylindroid curve. The articular faces of the centra are rather wider than deep, though nearly circular and biconcave. The ulna and radius are sub-quadrate. There is no third bone in the fore-arm. The phalanges are stout and but little compressed. The shoulder-girdle is on the Elasmosaurian type, with clavicles. The type species is *M. Leedsii*.*

In *Colymbosaurus* the neck is equally long. The neural arch and ribs are anchylosed to the centrum. The neurapophyses are lamellar and compressed from side to side. The centrum is biconcave, but the concavity decreases posteriorly. The articular surface is transversely,

* 'Geol. Soc. Quart. Journ.,' 1874, p. 197.

ovate at first, but afterwards deeper. The centrum is always wider than long, and has an oblique margin, which is absent in *Cimoliosaurus*. The humerus and femur are deeper than wide proximally. In the fore-arm there are three bones in a row, of which ulna and radius, like the tibia and fibula, are broader than long. There may sometimes be a fourth bone in this row (*C. Manselli*, Hulke sp.). The phalanges are not compressed. The types are from the Kimeridge Clay, and include *P. megadeirus* and *P. Manselli*.

These genera are distinguished by the extremities, though the vertebral articulation of the zygapophyses and many parts of the skeleton furnish differential characters.

Both genera are defined from *Polycotylus* and *Piptomerus* by the length of the dorsal centrum. The bevelled or rounded margin to the articular face of the centrum separates them from *Cimoliosaurus*. The absence of side-to-side compression of the centrum distinguishes them from *Elasmosaurus*. And they are separated from *Trinacromerum* by the structure of the glenoid cavity for the humerus, and the small number of uncompressed phalanges in the digits. Hence, without disregard of generic characters, and the facts of stratigraphical distribution, it seems impossible to follow the British Museum Catalogue, which enlarges the genus *Cimoliosaurus* to make it comprise all these Elasmosauridæ. And it will presently become evident that in *Muræenosaurus* the diversity of modification found in the clavicular arch is such as may define sub-genera within its present limits.

Notwithstanding the diverse aspects of the shoulder girdle in the Elasmosauridæ and Plesiosauridæ, and the circumstance that intermediate types are at present unknown, the difference between them is essentially in the fact that in all Elasmosaurians the supposed precoracoid region is ossified so as to come into median union with the coracoid by suture, and co-ossified with the scapula so as apparently to be an inseparable part of that bone; and it is these precoracoid portions of the scapulæ which alone meet each other in the median line, as do the precoracoid bones in *Procolophon* ('Phil. Trans.,' 1889, B, Pl. 9, fig. 9). In all Plesiosaurians, on the other hand, the precoracoid, if developed, remains cartilaginous; but I infer that a cartilage always extended from the anterior margin of the coracoid to the anterior extremity of the scapula, and by ossification of such cartilage the Plesiosaurian shoulder girdle would become Elasmosaurian.

§ 2. *The Clavicular Arch in the Elasmosaurians discovered by Mr. A. N. Leeds in the Oxford Clay.*

The clavicular bones may be placed anterior to the scapulo-precoracoids, partly under them on their visceral surface, but they never extend back to meet the coracoid bones, as in *Plesiosaurus*. Or they

may be wedged in the fork between the anterior termination of the scapulo-precoracoid elements. Or they may be entirely hidden from view, and lie upon the visceral aspect of the scapulo-precoracoid bones. These specimens are all in the Leeds Collection in the British Museum, or in that of Mr. A. N. Leeds at Eyebury.

They appear to me to show three types of structure:—

First, a clavicular arch formed of a large interclavicle with two clavicles forming its lateral wings, joined by squamous overlap, and not by suture.

Secondly, species in which the interclavicle is a V-shaped triangle, and clavicles are doubtfully present.

Thirdly, species in which two clavicles meet by median suture, without any indication of an interclavicle.

These modifications are such as might be expected to characterise genera rather than species, and they are accompanied by diversities in other parts of the skeleton.

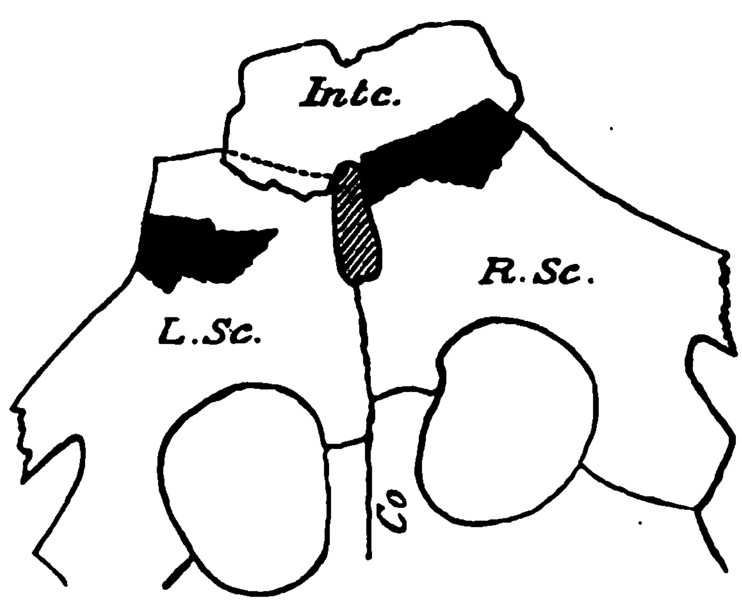


FIG. 6.—Part of the shoulder girdle and clavicular arch of *Muraenosaurus platyclis* from a drawing by Mr. A. N. Leeds, showing the position of the clavicular arch when found. *Co*, coracoid; *Sc*, scapula; *Intc.*, interclavicle and clavicles. The dark parts are missing. The light shading is a foramen.

(i.) In the first type the clavicular arch is formed substantially on the same plan as in the Lias genus *Plesiosaurus*, except that the clavicles rest upon the interclavicle by squamous overlap on its visceral surface, and their posterior-lateral prolongation is broken away. Yet when this surface is compared with that of *Plesiosaurus arcuatus* an almost identical T-shaped configuration of the interclavicle is exposed, while on the slightly convex ventral surface the clavicles are not seen at all in the specimen as preserved.

In the skeleton to which this specimen belongs the shoulder girdle is perfectly ossified. The transverse measurement over the humeral articulations is about 16 inches. The median processes of the coracoids are prolonged far forward, so as to make more than half

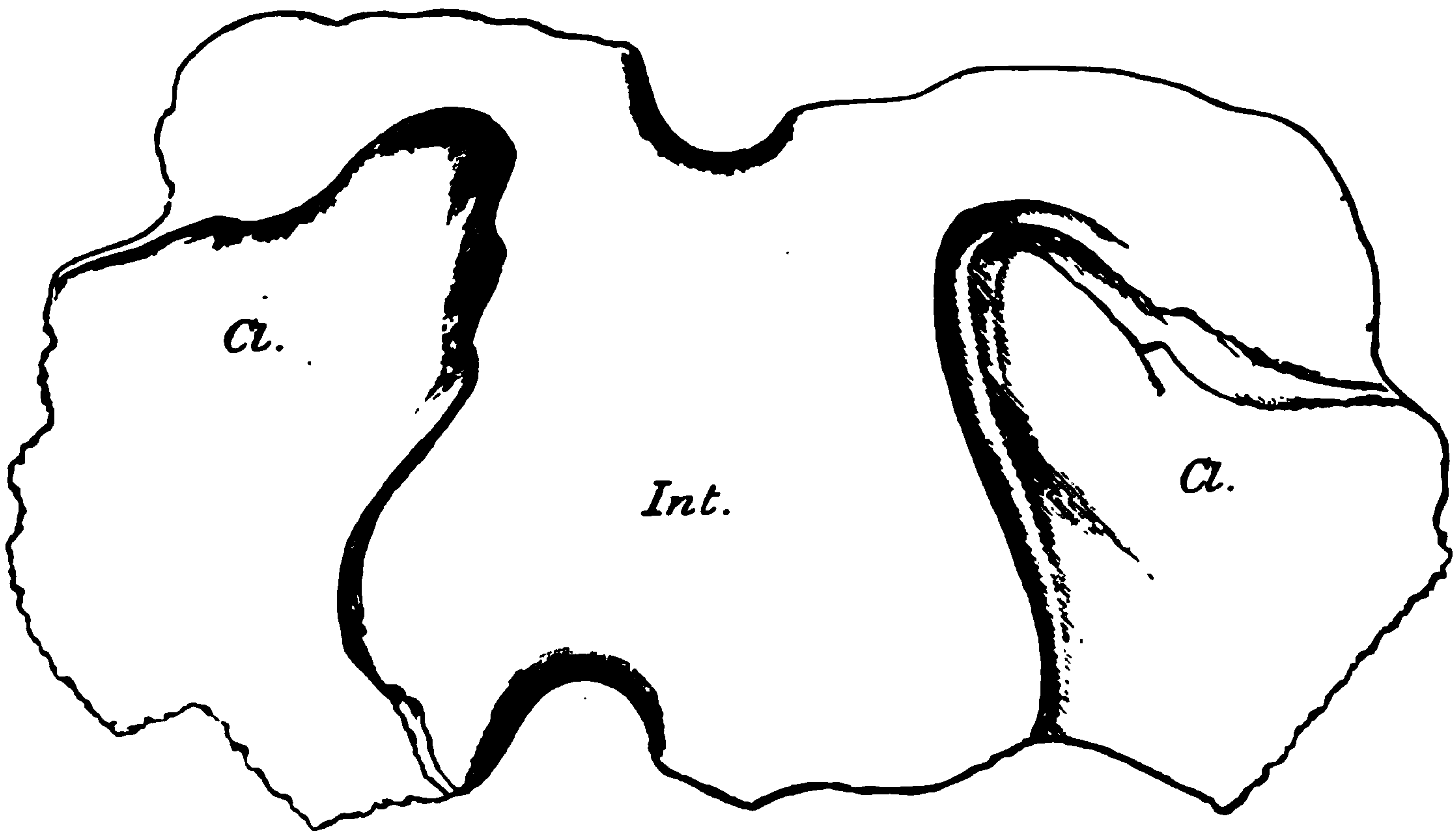


Fig. 7.—Clavicular arch of *Muraenosaurus platyclis*, showing *Cl*, the clavicles, resting upon *Int.*, the interclavicle.

the inner borders of the scapulo-coracoid vacuities. They terminate in transverse sutures, in advance of which the scapulæ extend for 8 inches, forming large wide flat plates, with oblique slightly concave anterior borders. These scapulæ meet in the usual way by a median suture for about 4 inches, anterior to which is a long median vacuity or foramen, $3\frac{1}{2}$ inches long and more than 1 inch wide, with sub-parallel sides, which is bounded in front by a posterior concavity in the interclavicle. A similar long median notch is seen between the scapulæ in the Leeds Collection (Brit. Mus.), No. 27, and in that specimen there is a similar, though smaller, interclavicle, more imperfectly preserved.

The anterior transverse bar of the interclavicle now described is defined by the clavicles which rest upon the bone. It is 7 inches wide. Owing to the contour of the clavicles, its lateral halves increase in depth to about an inch as they extend outward. The concave median notch in the anterior border is less than an inch wide. It corresponds in form and size with the notch on the posterior border of the bone, but is rather shallower. Between these opposite concavities which indent the interclavicle the antero-posterior measurement is $2\frac{3}{4}$ inches. This median part of the bone, which forms the wide longitudinal bar between the clavicles, is $2\frac{1}{2}$ inches in transverse measurement anteriorly, but widens posteriorly to 4 inches. Owing to the way in which the lateral margins are concavely defined by the overlapping clavicles, all the contours are somewhat unsymmetrical from distortion.

The right and left bones are unequal in length as preserved: one

measures 3 inches from front to back, and the other an inch more. Their internal borders are concave and sinuous, recalling the clavicles of *Plesiosaurus durobrivensis* already described. It is probable that the external processes of the clavicles now broken away were directed outward and backward, and in form similar to that species.

(ii.) A second Elasmosaurian clavicle, of different shape apparently, is preserved in the skeleton No. 23, in which 77 vertebræ were found. It has the vertebræ nearly flat at the articular ends, with the transverse measurement and depth of the centrum similar. Neither the neural arches nor cervical ribs are anchylosed, but both have relatively deep attachments.

In this species only one clavicle is preserved, but its form is perfect. One half of the other clavicle was found, but no trace of an interclavicle, though I suppose both of these bones to have rested upon the interclavicle, much as in the species just described. The bone is triradiate, 4 inches long and as wide. Its inner margin is the shortest, and is concave and slightly irregular. The superior and

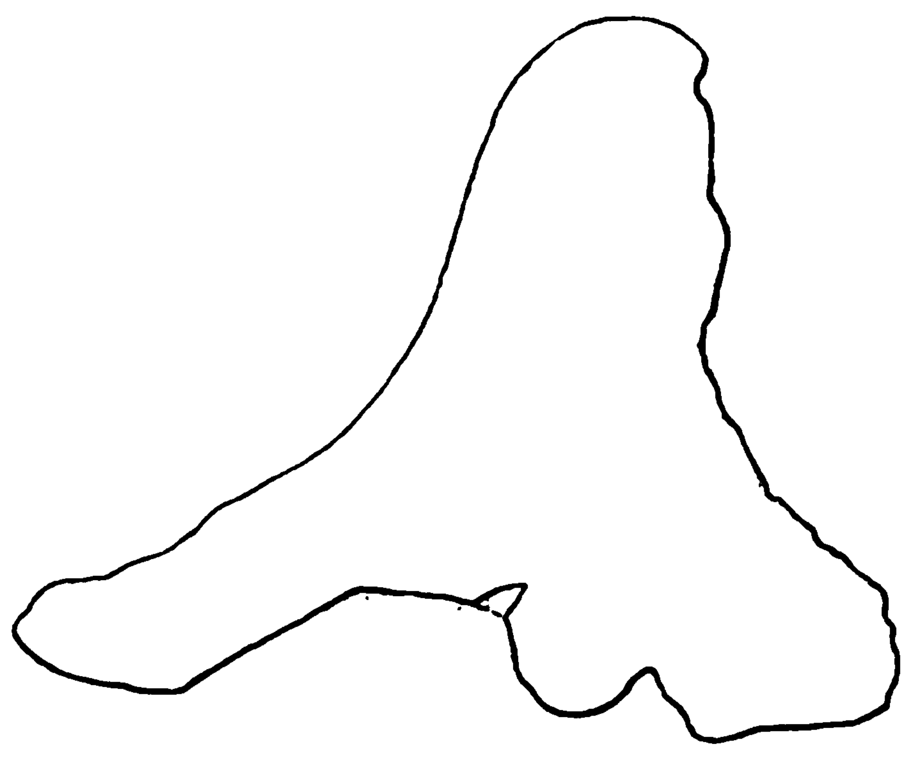


FIG. 8.—Clavicle of *Murænosaurus* (sp.).

inferior processes are about twice as wide as the external process, which is relatively long and slender. The anterior margin is concave. What I suppose to be the posterior margin is also concave, but a rounded prominence occurs on its inner third, and breaks the contour into a long external curve and a small inner notch.

The external termination is slightly widened and obliquely truncated, as though for attachment.

(iii.) A third form of clavicular arch, which appears to be probably of the same type, is represented by the imperfectly preserved interclavicle in the skeleton No. 26 in the Leeds Collection (Brit. Mus.). The scapulæ in this specimen are badly preserved, but they

have the external ascending process elongated rather more than in other specimens.

The interclavicle appears to have been sub-reniform, but its margin is imperfect all round. Its thin condition is more like that of a clavicle. It is $4\frac{1}{2}$ inches wide, and $3\frac{1}{2}$ inches deep. It shows radiating

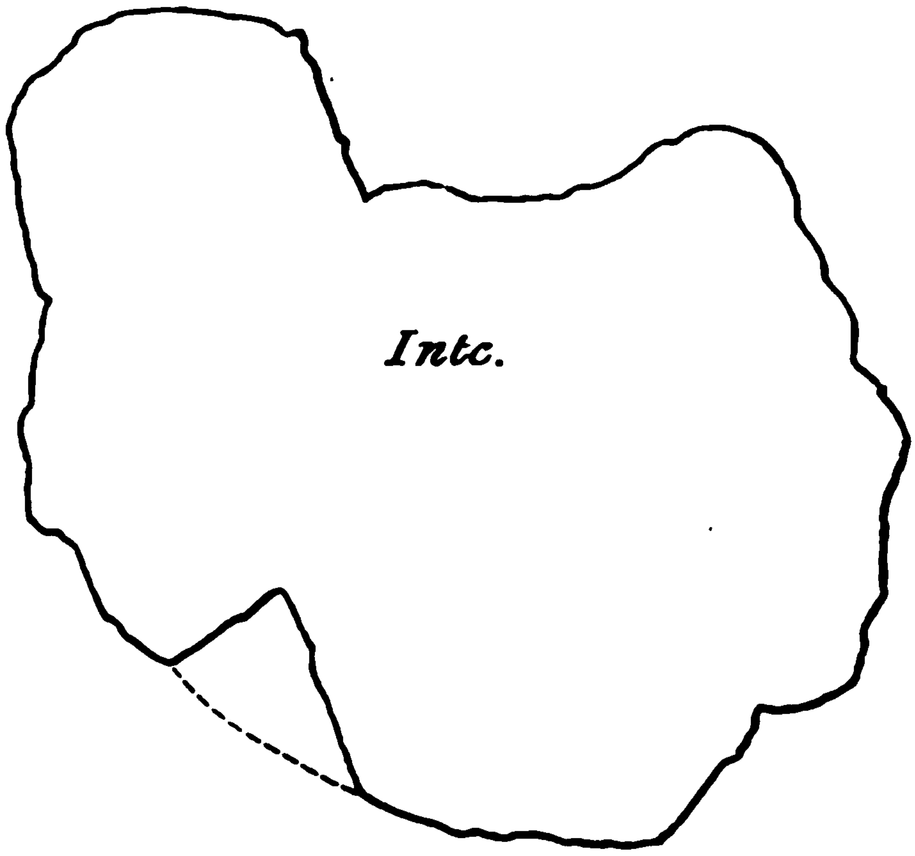


FIG. 9.—Imperfect interclavicle of *Muraenosaurus* (sp.).

lines of growth, and there are no indications of contact with other elements of the clavicular arch. If the bone is correctly determined, it shows an interesting modification of the interclavicle.

Oxford Clay Elasmosaurians show two types of variation from the kind of clavicular arch now described. One consists in the approximation of the clavicles, so that they articulate, and in this type there is no evidence of an interclavicle. In the other modification the interclavicle persists, wedged between the scapulæ, and the clavicles are probably not represented, or present as delicate films which have not been perfectly preserved.

(iv.) The type in which the clavicular arch reaches the smallest dimensions known to me is in the private collection of Mr. A. N. Leeds. Its remains comprise the shoulder girdle, bones of the fore limb, and some cervical vertebræ. A vertebra which is from the middle of the neck, and believed by Mr. Leeds to be about the 15th, has the centrum transversely ovate, $1\frac{3}{4}$ inch wide, $1\frac{1}{8}$ inch deep, and $1\frac{1}{4}$ inch long. The articular face is slightly concave, and margined by a narrow border. The ribs and neural arch are anchylosed to the centrum. The neural spine is compressed and somewhat elongated rising $4\frac{1}{2}$ inches above the base of the centrum.

The shoulder girdle is perfectly preserved. The least transverse measurement over the articular surfaces for the humerus is under

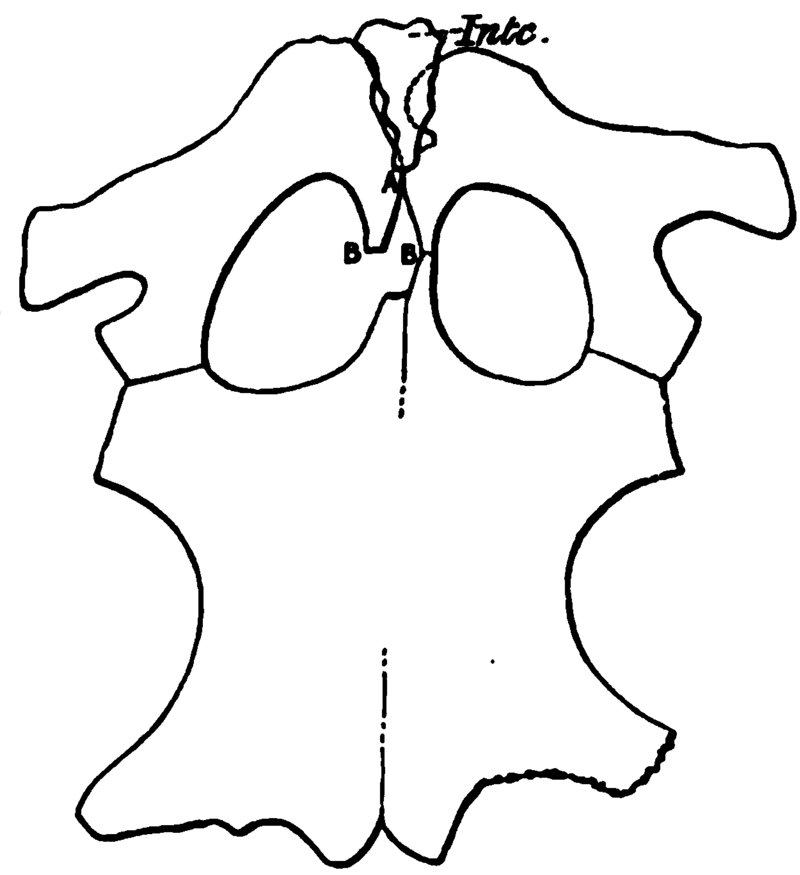


FIG. 10.—Shoulder girdle and interclavicle of *Muraenosaurus beloclis*. *Intc.*, Interclavicle. Scapulæ slightly distorted at their union with anterior processes of the coracoids.

10 inches. The antero-posterior measurement of the coracoids is 11 inches in the median line, and that of the scapulæ in the median line is $3\frac{3}{4}$ inches. The scapulæ are exceptionally slender, since the least width from the concave lateral border to the foramen is $1\frac{1}{2}$ inch. The least transverse width of the coracoids in the middle of their concave sides is $6\frac{1}{2}$ inches. The transverse measurement of the scapulæ behind the ascending process is $9\frac{1}{4}$ inches. The ossification between the anterior median margins of the scapulæ is not complete, and as they extend outward they are convexly rounded.

The interclavicle was found *in situ*, resting on the visceral surface in a depression between the anterior margins of the scapulæ and not projecting in advance of those bones. It is lanceolate in contour, $2\frac{3}{4}$ inches long, $1\frac{3}{4}$ inches wide towards the slightly concave anterior margin, and half as wide at the rounded posterior extremity. It is a little distorted, like the other bones of the shoulder girdle, has a flat visceral surface, and an angular ventral surface, due to the bone being traversed by an elevated median ridge, which dies away anteriorly, and from this ridge the lateral surfaces are inclined. On the left side of the ventral surface its middle part is covered by a thin film of bone, which I suppose may be part of the clavicle. It corresponds in texture and thickness with a detached film of bone which rests upon the right scapula. That ossification is triangular, about $1\frac{1}{4}$ inch in each measurement, and has nearly straight sides. It is quite separate from the interclavicle, and lies towards the external border of the scapula; there is no surface for its articulation, for all the margins of the interclavicle are sharp, thin, and perfectly ossified, like its

median crest. It is therefore probable that the clavicles were either loosely articulated to its margin, or extended between the interclavicle and scapula.

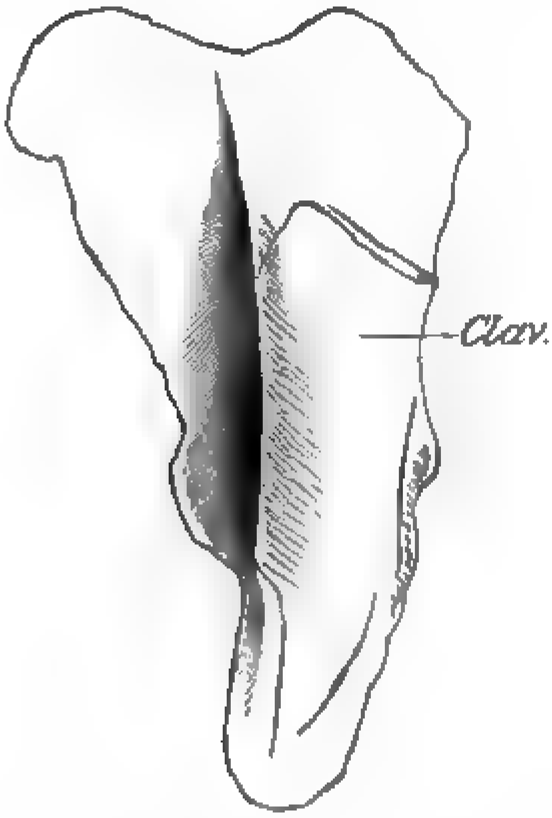


FIG. 11.—Ventral aspect of interclavicle, *Muraenosaurus beloclis*; *Clav.* may be a portion of the thin left clavicle upon its ventral surface.

There is no other example of an interclavicle received between the scapulo-precoracoids as in this species, for the anterior notch between these bones in advance of their sutural surfaces is not unlike the notch already described under the heading (i), except that it is less well defined and more irregular and narrower, and it is into this notch that the base of the interclavicle is articulated. In its posterior part the surrounding bone is thick, forming a concave channel on each side, which is limited in front on the visceral surface by a tubercle, anterior to which is a small transverse notch on each side in the scapular bone, which becomes thin as it extends forward, thus making the clavicular cavity +-shaped. There is every appearance of cartilage having extended between the opposite scapular margins, so that the interclavicle may have been hidden upon the ventral surface, and the anterior part of that bone may not have been in actual contact with the thin scapular plate in front of it. The position of the interclavicle appears to show that it ossified prior to the bones between which it is placed.

In this series of Elasmosaurians there is seen a remarkable change in the condition of the clavicular arch. In the first species described it is large and much broader than long, and placed behind the

scapular bones. But in this species it has become small, is much longer than wide, and placed between the scapulæ in a way which shows that it might by further decrease entirely disappear, or when ossification obliterates the median suture it may become embedded between the lateral ossifications of the precoracoid region, and cease to be recognisable. But the clavicles might still persist on the visceral surface of the scapulæ if such a change took place.

I refer all these types in which clavicles and interclavicle are developed, and connected in the way described, to the genus *Murænosaurus*, of which the type has been already described.* In all these species the ulna and radius, and tibia and fibula, are approximately equal and sub-quadrate bones, usually with the radius and tibia slightly the larger, meeting each other in both limbs, and enclosing a foramen between what were in *Plesiosaurus* the long concave sides of the bones. In the species last described there is an interesting tendency, though a slight one, to vertical elongation of the radius and transverse elongation of the ulna, both bones being about 2 inches wide, while the radius is $2\frac{1}{4}$ inches long and the ulna $1\frac{7}{8}$ inch long. The humerus in this type is 7 inches long and 4 inches wide, with well-ossified facets for the radius and ulna, which are mutually inclined, and meet at a sharp angle.

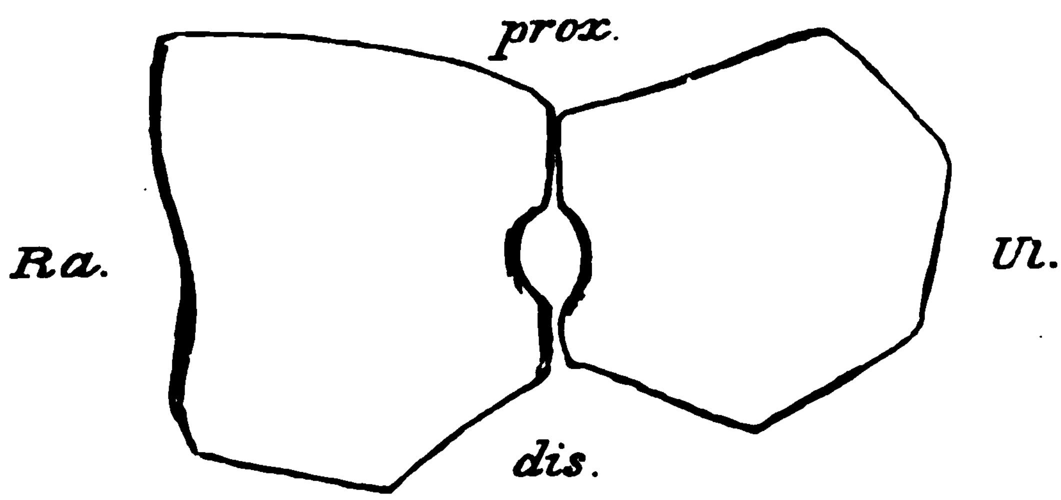


FIG. 12.—Radius and ulna of the same specimen. *Ra.*, radius; *Ul.*, ulna; *prox.*, proximal margin; *dis.*, distal border.

(v.) The specimen in the Leeds Collection (Brit. Mus.) numbered 31 has been referred to *Cimoliosaurus eumerus* (Phillips species), ('Cat. Foss. Rept. and Amph., Brit. Mus.,' Part II, p. 205); but the different forms and proportions of all the limb bones justify its separation, and as a sub-genus of *Murænosaurus* it is named *Cryptoclidus platymerus*. As compared with *Murænosaurus Leedsii* (No. 25, Leeds Coll., Brit. Mus.), it has the centrum broader, shorter, and more

* 'Geol. Soc. Quart. Journ.,' 1874, p. 197. At that time the shoulder girdle was only known from fragments; and the account now given of the scapulæ corrects the conjectural restoration which was based on that imperfect evidence.

concave. The cervical and caudal ribs and neural arch are anchylosed to the centrum. The cervical neural spine is short. The zygapophyses are rather less cylindroid. The scapulæ are unfortunately imperfect; enough is preserved to show that they were wide anterior to the scapulo-coracoid foramina, but not enough to show how they terminated in front. The coracoids are large, and have the postero-lateral prolongation of the bone well developed, as in *Colymbosaurus*.

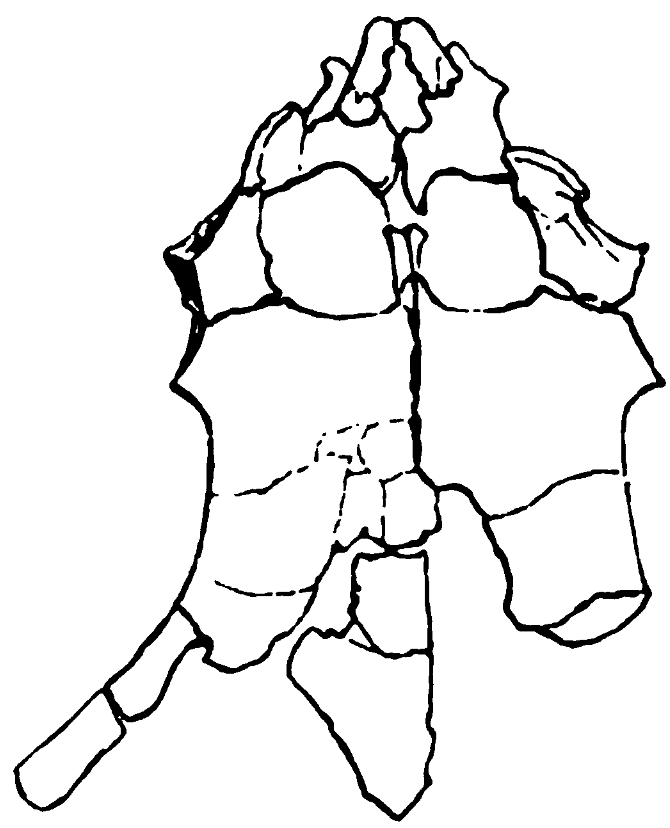


FIG. 13.—Shoulder girdle of *Murænosaurus* (*Cryptoclidus*) *platymerus*. *c*, clavicles.

There are two bones found with this specimen which I regard as clavicles. Unlike other specimens,* they unite with each other by an ovate suture, which is from half to three-quarters of an inch long, and they are inclined to meet each other anteriorly at an angle of 45°, which is about the same as the angle of inclination of the scapulæ. The left clavicle is an oblong plate $4\frac{1}{4}$ inches long as preserved, but imperfect on both the posterior and internal margins. The right fragment is $3\frac{1}{4}$ inches long. The anterior end is truncated, and hardly extends beyond the articulation, where the transverse measurement of the bone is $1\frac{1}{2}$ inch. Just behind the articulation, the inner border has a concavity more than half an inch long, notching out the border in both specimens; but behind the notch the bone is broken away. Its smooth external border is slightly concave in length, and is prolonged diagonally outward and backward. The width of the left plate at the posterior fracture is about 2 inches.

* Mr. Leeds informs me that he has since obtained another type in which the two triangular clavicles meet in the median line, without trace of an interclavicle.

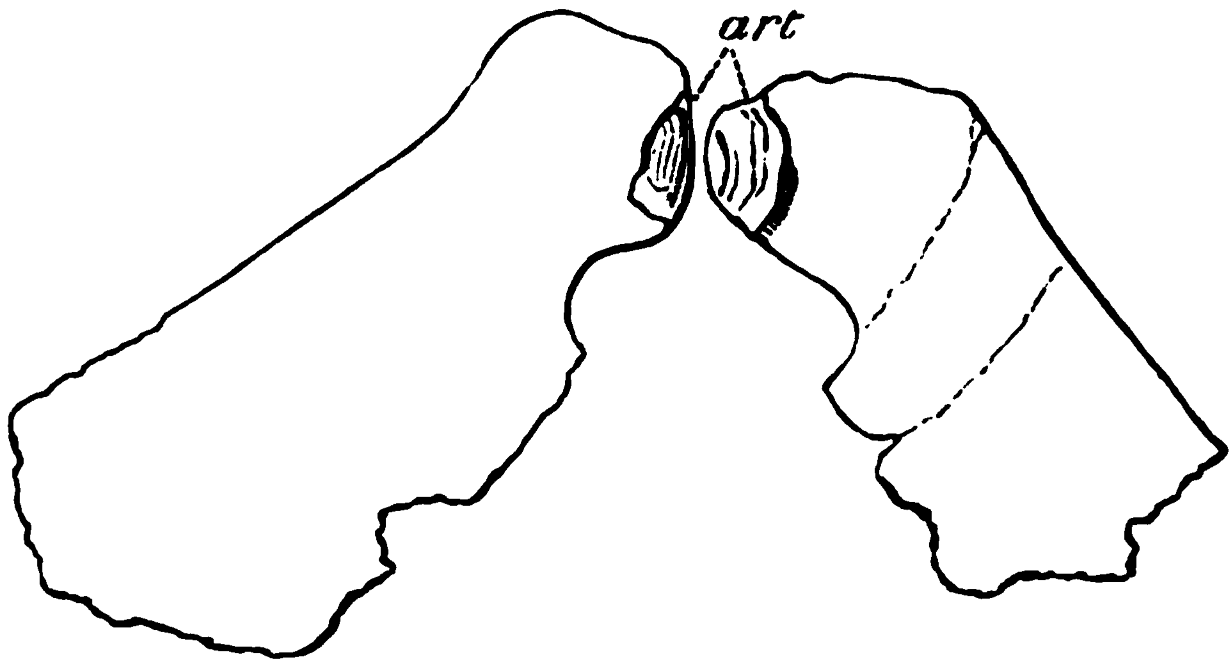


FIG. 14.—Clavicles of *Cryptoclidus platymerus*. *art*, median articular surface.

On the visceral surfaces of the scapulæ are shallow depressions terminating outward in a sharp angle such as might have received the external processes of clavicles like those found with the Leeds Collection specimen No. 36. These impressions are symmetrical and seen in both scapulæ, and so far they support the interpretation of this clavicular arch now offered. No evidence of an interclavicle was met with, and there is no evidence of its existence.

If the clavicles are correctly identified, their mode of occurrence may account for the circumstance that they have not been observed in *Colymbosaurus*. And their sutural union as in a Chelonian may be regarded as a generic character, separating this type from *Murænosaurus*.

An important generic character is found in great vertical depth of the radius and transverse elongation of the ulna. It has been stated that the ulna in another specimen consists of two separate bones* ('Cat. Foss. Rep. Brit. Mus.,' Part II, p. 206), but I have been unable to detect evidence of a suture between them, and regard the division as a fracture. Extreme as is the divergence in proportion, these bones are better compared with those of *Murænosaurus* than any other type. The separation of the ulna from the olecranon characterises the genus *Colymbosaurus*, and is seen in *C. Manselli* (Hulke sp.), in which the bones are in close union, in *C. megadeirus* apparently, and in *C. Portlandicus* (Owen sp.), in which the bones are ovate, less perfectly ossified, and separate. This species, No. 31, may be a precursor of *Colymbosaurus*, but there is no reason for referring it to that genus, which has the humerus long and narrow. The first row of the carpus appears to have originally comprised five bones, as in the Mesosauria; but the small inner and outer elements are now blended with the carpals next them, reducing the number of

* The fragments, which had been mended, have been separated.

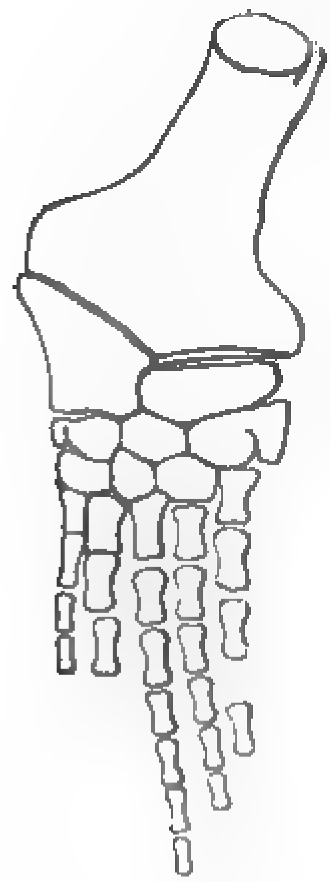


FIG. 15.—Fore limb of *Muraenosaurus* (*Cryptoclidus*) *platymerus*.

separate bones to three. There are only three carpals in the second row, though there may be a rudimentary fourth bone on the anterior margin.

The first digit is slender and short in this species, and all the phalangeal bones are sub-cylindrical, showing, as in other species of *Muraenosaurus* no trace of the compression which characterised *Cimoliosaurus*.

It is not improbable that, with fuller knowledge, the conceptions of genera here indicated may, in some cases, be modified; but, till better examples of the American genera are found and figured, it will be difficult to contrast them with those now described, and make the definitions exact.

IV. Classification.

Characters of value in classification show gradations of development in the Sauropterygia. This is conspicuous in the size and form of the head, the relative length of the neck, the mode of articulation of the cervical ribs by two heads or by one, or by anchylosis, the length of the centrum in relation to its breadth in the several regions of the vertebral column, the form and mode of attachment of the neurapophyses, the form of the zygapophyses, the structure of the shoulder girdle, the forms and conditions of the mesopodial

bones, and the arrest or development of the process of ossification in the various elements of the skeleton.

Of all these characters the last is the most difficult to value, for there is some evidence tending to the inference that ossification became better developed with the progress of geological time, and surfaces which in Liassic types had always retained the cartilaginous condition of immaturity, in Cretaceous types show the completed ossification of old age. This condition is not, however, universal, since *Eretmosaurus rugosus* has ossification perfected in a way not known in other Liassic genera, and *Stereosaurus platyomus* of the Cambridge Greensand has the vertebræ crowded together irregularly, while the extremities of the short, wide, thick propodial bones remain unossified.

And it is remarkable that many Liassic species have the articular faces of the vertebral centra deeply biconcave, while in many Cretaceous species those surfaces are nearly or quite flat; in the shoulder girdle nothing but continued ossification apparently is needed to convert the Liassic Plesiosaurian into the Oolitic and Cretaceous Elasmosaurian type. *Eretmosaurus* is the nearest approach to this type, known from the Lias.

It thus appears as though some animals complete their embryology early in life, others at intervals during life, while in most types the embryonic development takes place gradually during successive epochs of geological time, giving rise to classification of its stages, indicated as genera, families, orders; and therefore that the young individuals of a late period of time simulate genera of an earlier age.

The character which appears to be most important in Sauropterygia as a ground for primary classification is the presence of two facets, or one facet, on the side of the centrum for the articulation of the cervical rib. If two are present, both facets are upon the centrum, and exhibit many degrees of approximation, seen in *Rhomaleosaurus*, *Pliosaurus*, *Plesiosaurus*, before the division becomes obliterated in *Murænosaurus*, *Colymbosaurus*, and the Cretaceous types. This condition is of further interest, from the fact that among existing Vertebrates a similarly divided articulation for the rib upon the centrum is only known in the existing Urodele Amphibia. Most, if not all, of the Plesiosauridæ have the rib facet transversely cleft; while no Elasmosaurian is at present known in which the same condition is found. So that a division may be made into groups with ribs of the Y-type and I-type. The former sub-division includes two extreme modifications, one with a long neck, which is well represented in the Lias by *Plesiosaurus homalospondylus* and *P. dolichodeirus;* and a type in which the head becomes larger and the neck shorter, represented by *Rhomaleosaurus Cramptoni* in the Lias and *Pliosaurus* in the Oxford and Kimeridge Clays.

The short-necked genera are distinguished as a group by having the two articular costal facets placed chiefly at the sides of the centrum, and not at its infero-lateral angle. This circumstance appears to indicate that the neck is elongated chiefly by the addition of vertebræ to its anterior portion. The mesopodial bones are two in number, and more or less quadrate, with a tendency to transverse extension in the ulna. The dorsal vertebræ are relatively long compared with those of the neck. In *Rhomaleosaurus* the average length of the cervical centrum is 2·66 inches, while the average length of the dorsal vertebræ is 3·2 inches. In *Rhomaleosaurus* the facets remain separated to the end of the series, but in some species of *Pliosaurus* there is an approximation of the facets in the posterior cervical vertebræ which is not seen further forward. The relatively small size of the head in *Rhomaleosaurus* as compared with *Pliosaurus* shows that the head is not necessarily long in all the short-necked genera. The shoulder girdle in the Rhomaleosaur being unknown at present, there is no means of comparing it with that attributed to *Pliosaurus*.

The long-necked genera have the cervical vertebræ in greater number, and relatively longer, and, except the earliest, they are usually as long as the dorsal vertebræ. The articulations for the cervical ribs are elongated from front to back, longitudinally divided, but usually so compressed from above downward that the division is only a narrow shallow channel, always placed at the infero-posterior angle of the centrum. The two facets are obvious in species like *P. dolichodeirus*; in many others they are only to be recognised by careful examination. In this genus the radius is elongated, with its lateral borders concave and ossified, and the distal end narrower than the proximal end.

The scapulæ and coracoids never meet in the median line, unless in the genus *Eretmosaurus*. This condition has been figured in the British Museum specimen 2041 ('Geol. Soc. Quart. Journ.,' 1874, p. 446), where a wide interspace is left between the coracoids and scapulæ in the median line in front, and there is a roughness upon the scapula as though the interclavicle or clavicle had extended upon it. The interclavicle usually completes the inner border of the coracoid foramen in *Plesiosaurus*; but in this fossil the relations of the bones are like those attributed to *Pliosaurus* by Sir R. Owen. According to Mr. Lydekker, what I regard as the pre-articular part of the scapula is the humerus of *P. Hawkinsi* ('Cat. Foss. Rept. Brit. Mus.,' Part II, p. 277). This is a matter that may be definitely determined by examination of the specimen, which comprises the scapula only, closely united by suture to the coracoid.

In the second division of the Sauropterygia or Elasmosauridæ the cervical ribs articulate by a single head with the centrum, the scapulæ, as well as the coracoids, meet each other in the median line, the clavicles, so far as they are known, are usually slender, the meso-

podial bones are quadrate or transversely elongated, and the carpals and tarsals are transversely oblong. There is a certain parallelism between this group and the Plesiosauridæ, as though it had been modified from it, and the genus *Polycotylus* in the shortness of its centrum has some resemblance to *Rhomaleosaurus*, just as in length of neck and vertebræ *Muræenosaurus* resembles *Plesiosaurus*.

This scheme of classification is summarised in the following table:—

SAUROPTERYGIA.

Aquatic Sauromorpha, with the extremities modified for swimming only. They are divided into two groups:—

DICRANOPLEURA comprise all genera with fork-headed cervical ribs. They are divided into two groups:—

Dolichodeira (or Plesiosauridæ) comprise the long-necked genera—

Plesiosaurus, in which the carpus is weakly ossified.

Eretmosaurus, in which the carpus is strongly ossified and the scapula appears to have an inner and an outer union with the coracoid.

Brachydeira (or Pliosauridæ), the genera with short necks—

Rhomaleosaurus, in which the cervical vertebræ are shorter than the dorsals. Articular face concave.

Pliosaurus, in which the cervical vertebræ are shorter, and flat on the articular face.

CERCIDOPLEURA comprise all genera with single-headed, or skewer-like, cervical ribs. They are not yet divided into groups, and are all included in the Elasmosauridæ—

Polyptychodon.
Polycotylus.
Cimoliosaurus.
Stereosaurus.
Mauisaurus.
Elasmosaurus.
Trinacromerum.
Colymbosaurus.
Muræenosaurus.
Cryptoclidus.

Report of the Kew Committee for the Fourteen Months ending December 31, 1891.

The operations of The Kew Observatory, in the Old Deer Park, Richmond, Surrey, are controlled by the Kew Committee, which is constituted as follows:

Mr. F. Galton, *Chairman.*

Captain W. de W. Abney, C.B., R.E.
Prof. W. G. Adams.
Staff-Commander E. W. Creak, R.N.
Prof. G. C. Foster.
Admiral Sir G. H. Richards, K.C.B.
The Earl of Rosse, K.P.
Prof. A. W. Rücker.
Mr. R. H. Scott.
Lieutenant-General R. Strachey, C.S.I.
General J. T. Walker, C.B.
Captain W. J. L. Wharton, R.N.

The work at the Observatory may be considered under the following heads:—

1st. Magnetic observations.
2nd. Meteorological observations.
3rd. Solar observations.
4th. Experimental, in connexion with any of the above departments.
5th. Verification of instruments.
6th. Rating of Watches and Marine Chronometers.
7th. Miscellaneous.

I. Magnetic Observations.

The magnetographs have worked satisfactorily all through since last report. The curves obtained, representing Declination, Horizontal Force, and Vertical Force, have shown a marked increased activity in terrestrial magnetic changes as compared with the preceding year, although no very large disturbances have been registered.

The principal movements occurred on the following dates, viz.:—

March 2—3, and 31, April 8 and 12, May 14, 15, and 16, June 14, September 9—12, October 24, November 20—21, and December 7.

In accordance with the usual practice, determinations of the scale values of all the instruments were made in January last, and the ordinates for the different photographic curves were then found to be as follows:—

Declinometer: 1 inch = 0° 22′·04. 1 cm. = 0° 8′·7.

Bifilar, January 7, 1891, for 1 inch δH = 0·0277 foot grain unit.
,, 1 cm. ,, = 0·00050 C.G.S. unit.

Balance, January 8, 1891, for 1 inch δV = 0·0275 foot grain unit.
,, 1 cm. ,, = 0·00050 C.G.S. unit.

The following are the principal results of the Magnetic Elements for the years 1890 and 1891:—

Mean Westerly Declination..	1890	..	17° 50′·6
,, ,, ..	1891	..	17 41·9
Mean Horizontal Force......	1890	..	0·18173 C.G.S. unit.
,, ,, ...	1891	..	0·18193 ,,
Mean Dip..................	1890	..	67° 32′·5
,, ...	1891	..	31·2
Mean Vertical Force........	1891	..	0·43962 C.G.S. unit.

Additional observations of the Horizontal Force, Inclination, and Declination have been made each month with the absolute instruments for the purpose of determining with greater precision the zero values of the magnetograph curves.

Information on matters relating to terrestrial magnetism and various data have been supplied to Professors Thorpe and Rücker, Dr. Van Rijckevorsel, Captain Schück, and Professor Stroud.

In January the Kew 9-inch unifilar, by Jones, was sent to Messrs. Elliot Brothers for cleaning, and, at the same time, certain alterations were introduced in order to modernise the instrument. Unfortunately, the heavy brass telescope support was found on examination to be slightly magnetic, and it was therefore discarded, and, on February 12, the magnetometer was restored to its original state, with the exception of a brass tie-piece, which, being found non-magnetic, was retained until the end of the year.

On closely discussing the Declination observations of the preceding years it was found that observations given with the old collimator magnet N.E. were more subject to a variable torsion effect than were those of the collimator marked K.O. 90. The two having been employed in conjunction for one year, it was decided, with the commencement of 1891, to discard altogether the use of the old heavy magnet for the purpose of observing Declination.

II. Meteorological Observations.

The several self-recording instruments for the continuous registration respectively of Atmospheric Pressure, Temperature, and Humidity, Wind (direction and velocity), Bright Sunshine, and Rain have been maintained in regular operation throughout the year, with the exception of the wet-bulb thermograph.

The readings of the last-named instrument during the winter of 1890–91 became irregular, and it was found to vary considerably from its accompanying standard. It was accordingly decided to dismount the thermometer and to replace it by a new tube, which was done in July last. On examination, the bulb showed the existence of a crack, which eventually extended completely around it.

The scale value of the new tube has been determined by means of nearly 300 comparative readings, and new glass and ivory tabulating scales for it have been constructed at the Meteorological Office.

For controlling these values, an experimental determination of the zero of the instrument was made by means of melting ice.

Experiments were made, unsuccessfully, to use a Richard pen with the Beckley rain gauge, but a BBB black-lead pencil was found to be more reliable in its indications than such a pen.

The standard eye observations for the control of the automatic records have been duly registered.

The tabulations of the meteorological traces have been regularly made, and these, as well as copies of the eye observations, with notes of weather, cloud, and sunshine, have been transmitted, as usual, to the Meteorological Office.

With the sanction of the Meteorological Council, data have been supplied to the Council of the Royal Meteorological Society, the editor of 'Symons's Monthly Meteorological Magazine,' Dr. Rowland, and others.

Tables of the monthly values of the rainfall and temperature have been regularly sent to the Meteorological Sub-Committee of the Croydon Microscopical and Natural History Club for publication in their Proceedings. Detailed information of all thunderstorms observed in the neighbourhood during the year has been forwarded to the Royal Meteorological Society, soon after their occurrence.

Atmospheric Electricity.—The electrograph has been maintained in action during the greater portion of the year. The records were, however, lost for forty-eight days on account of the freezing of the water-jet during frost in winter.

The instrument has failed in sensibility during the last year owing to the large extent of diminution which the 60-cell chloride of silver battery has experienced in its charge, the potential of which has apparently diminished by one-half.

This having been reported to the Meteorological Council, it was decided by them to request Professor J. J. Thomson to examine into the subject of the measurement of Atmospheric Electricity, and, meanwhile, to continue the instrument in action in its present condition.

III. Solar Observations.

Sun-spots.—Sketches of Sun-spots have been made on 170 days, and the groups numbered after Schwabe's method.

Time Signals.—These have been received from Greenwich through the G.P.O. with regularity since last report, with the following exceptions:—

On six occasions, viz., March 10; May 30; August 18, 19, and 27; and September 11, no signal was received either at 10 A.M. or 1 P.M. On January 14 and February 4 it arrived two seconds late, and on October 8, 9, and 10 it did not record itself on the chronograph, but was only observed by the galvanometer.

Transit Observation.—Occasional solar and sidereal transits have been observed as checks upon the Greenwich signalled times.

Violle's Actinometer.—The copies of the observations made during 1890 were duly forwarded to the Meteorological Office in January, and, as the Committee understand, have been handed over by that Office to Mr. H. F. Blanford, who will report on the subject to the Solar Physics Committee.

IV. Experimental Work.

The Committee have had under trial on the roof of the Observatory two new forms of wind registering instruments, the anemo-cinemograph of MM. Richard Frères, of Paris, and the sight-indicating velocity meter by Munro, of London.

The first-named instrument is an improved form of the old windmill vanes anemometer which was used by Smeaton after Rouse and Robins, but is best known as Whewell's. The anemo-cinemograph is similar to that which was employed on the top of the Eiffel Tower at Paris, and the vanes, by running constantly against a train of clockwork, record directly on a sheet of paper the velocity of motion of the wind at any time. Continuous records were obtained for six months, and the result given would seem to show that the indications of the Kew Beckley anemograph are in excess of those given by the new instrument. These are 20 per cent. less than those of the anemograph with winds blowing at 40 miles or upwards per hour, and 12 per cent. less with light winds which blow at from 6 to 10 miles

per hour. A reduction of the Robinson factor from 3 to 2·5 would serve to render the readings of the two instruments more nearly comparable.

As the Richard instrument is designed to record the velocity of the wind in gusts as well as the total run during any definite interval, no detailed comparisons with the Robinson indications are possible; but it may be noted that during the period the cinemograph was under observation gusts of 45 and 43 miles per hour were recorded, whilst simultaneous curve readings of the Robinson gave hourly rates of 55 and 52 miles for quarter of an hour intervals.

The Munro sight-indicating anemometer is a sensitive Robinson cup arrangement, which drives by means of a small centrifugal pump a column of oil up a glass tube. Its height above a fixed zero mark, as shown on a divided porcelain scale at the side, indicates the velocity of rotation of the cups when converted into miles per hour of wind movement. The divisions of the scale have been laid down in accordance with Mr. Dines' experimental deduction. When the instrument was originally set up, it was found incapable of recording a velocity of more than 40 miles per hour, but, during a gale in November, velocities were attained during several gusts of over 70 miles per hour, and accordingly Mr. Munro has found it desirable to change the gearing of the pump so as to enable the higher values to be indicated. The comparisons with the new gearing are not sufficient in number to furnish results suitable for quotation at the present time, but they appear to show, during gusts, rates fully 20 per cent. higher than the cinemograph gives.

The instrument as fitted at present fails to work during frost, owing to congelation of the oil employed.

Dr. E. Van Rijckevorsel, of Rotterdam, visited the Observatory in July for the purpose of making simultaneous magnetic observations with the Kew, his own, and the Utrecht unifilar magnetometers, and of comparing the results with those he had recently made with the magnetometers in use at the Observatories at Parc St. Maur, Wilhelmshafen, and Utrecht.

Professor Rücker has also been investigating the differences found to exist in similar simultaneous readings of his three unifilars; and his assistants, Messrs. Gray and Watson, have visited Kew on numerous occasions in order to make the necessary observations.

Cloud Photographs.—The operations with the cloud cameras have been conducted during the past year solely according to the simplified method of zenith observation, as described in last year's report, and results were obtained on 24 days. A joint paper by General Strachey and the Superintendent, describing the plan of working, was read before the Royal Society in June, and was fully illustrated by photographs shown in the optical lantern. A report, giving a detailed

account of the year's work, was forwarded to the Meteorological Council in November last.

Particulars, with specimens of cloud pictures, were also supplied to Mr. Rotch, of the Blue Hill Observatory, for communication to the Committee of the International Meteorological Conference at Munich.

Experiments were also made with several new lenses kindly lent by Mr. Dallmeyer, in order to select one suitable for giving pictures covering a wider field of view than the R.R. lens hitherto employed, which confines the observer to clouds within 15° of the zenith.

The results of these experiments, as well as others with Eastmann films used instead of glass plates, have been communicated to the Meteorological Council.

In compliance with the request forwarded by Mr. Clayden, secretary to the British Association Committee on Meteorological Photography, for copies of photographs illustrating meteorological phenomena, or their effects, the Committee forwarded a selection of duplicate cloud and other photographs to be added to the collection which has been formed.

V. Verification of Instruments.

The following instruments have been purchased on commission and their constants determined:—

1 Unifilar magnetometer for the Royal Observatory, Greenwich.
1 Ditto, ditto for the Vatican Observatory, Rome.
1 Ditto, ditto for the Meteorological Department, Brazil.
1 Inclinometer, ditto, ditto.
1 Ditto has been repaired for the Hague.
1 Electrical anemometer for the Observatory, Mauritius.
1 Marine chronometer for the Colába Observatory, Bombay.
1 Richard aneroid, and set of thermometers, for the Richmond Terrace Gardens Committee.
1 Telescope.
1 Set of magnetograph needles for Mauritius Observatory.
1 Ditto dip needles and bar magnets for the Hague.

The total number of other instruments compared between November 1, 1890, and December 31, 1891, was as follows:—

Air-meters	7
Anemometers	19
Aneroids	72
Artificial horizons	10
Carried forward	108

Brought forward	108
Barometers, Marine	111
,, Standard	57
,, Station	39
Binoculars	470
Compasses	22
Hydrometers	224
Inclinometers	3
Photographic Lenses	19
Magnets	2
Navy Telescopes	374
Rain Gauges	17
Rain Measures	39
Sextants	428
,, Shades	7
Sunshine Recorders	1
Theodolites	5
Thermometers, Arctic	133
,, Avitreous or Immisch's	231
,, Chemical	108
,, Clinical	15,692
,, Deep sea	58
,, Meteorological	2,289
,, Mountain	26
,, Solar radiation	1
,, Standards	62
Unifilars	3
Total	20,529

Duplicate copies of corrections have been supplied in 52 cases.

The number of instruments rejected on account of excessive error, or which from other causes did not record with sufficient accuracy, was as follows:—

Thermometers, clinical	57
,, ordinary meteorological	27
Various	132

10 Standard Thermometers have also been calibrated, and supplied to 4 applicants during the year; 6 are placed in stock.

There are at present in the Observatory undergoing verification, 9 Barometers, 452 Thermometers, 2 Hydrometers, 23 Sextants, and 15 Telescopes.

Sextant Testing.—The apparatus, consisting of two Mawson and

Swan's glow lamps, lighted by electricity derived from one of Pitkin's storage batteries, has been successfully employed in testing the dark shades of sextants, when requisite, during the past year. It has been found necessary to replace the lamps in two instances; but the initial charge of the battery has proved capable of working them throughout the whole twelvemonths without replenishing.

Telescope Testing.—A second test plate has been procured, and mounted on a portable frame, with a reflector, in order to enable the examination of telescopes to be prosecuted from the optical room as well as from the lawn. A detailed form of certificate has been prepared and issued with telescopes and binoculars examined for the general public.

6 Look-out telescopes have been examined and certified for the Brethren of the Trinity House.

Normal Thermometers.—M. Benoit, the Director of the Conservatoire des Poids et Mesures, Paris, having completed his examination of the three standard thermometers, and submitted his report upon them to the Committee, who have placed it in the hands of Professor Rücker for discussion, proceeded to examine the low-range alcohol thermometer which accompanied them. Whilst conducting this operation, M. Carpenter, the observer, was so unfortunate as to break the tube. M. Benoit, having strongly advised that further comparisons at low temperatures should be made by means of thermometers filled with toluene instead of with alcohol, has been requested by the Committee to order such an instrument of M. Tonnelot, the maker, and compare it with the Sèvres standards before its delivery in England. The mercurial standards were safely returned to the custody of the Observatory by M. Carpenter in May last.

VI. Rating of Watches.

During the fourteen months 709 entries of watches for rating were made. They were sent for testing in the following classes:—

For class A, 468; class B, 153; and class C, 86; subsidiary trial, 2

Of these 161 failed to gain any award; 49 passed with C, 140 with B, 327 with A certificates, and 29 of the latter obtained the highest, class A *especially good.*

In the Appendix will be found statements giving the results of trial of the 29 watches which obtained the highest numbers of marks during the year, the highest position being attained by Messrs. Stauffer, Son, and Co., London. This watch was a keyless tourbillon chronometer, with going barrel, which obtained the very excellent total 91·6 of marks out of a possible 100.

Marine Chronometers.—Certificates showing the mean daily rate

and the variations of rate at three different temperatures have been awarded to 18 marine chronometers after undergoing the 35 days' trial.

The Committee having had their attention drawn to the limited nature of their trials for first-class marine chronometers, decided to establish a second and more rigorous trial for these instruments, and have now organised two classes, which are as follows:—

Class A trial, extending over 55 days, comprising runs at temperatures of 45°, 70°, 95° Faht.; and Class B trials, which last for 35 days, and include readings at temperatures of 55°, 70°, and 85° Faht. only. For Class A tests, the individual runs are 10 days at each temperature; whilst for Class B tests, they are only 7 days each in duration.

The Committee have drawn up a special circular addressed to the directors of steamship companies, calling attention to rating chronometers, and have distributed it to the managers of all the principal companies of vessels sailing from British ports.

As the question of the rate of a chronometer under varying temperatures is intimately related to the behaviour of the lubricating material employed, when heated, the Committee asked Professor T. Thorpe, F.R.S., to favour them with his opinion as to the temperature to which a chronometer may be subjected to without producing a deleterious effect upon its oil.

Non-Magnetic Watches.—Owing to the extension of the use of electrical dynamometers, a class of watches provided with springs and balances of palladium or some alloy, and termed non-magnetic watches, has been brought into more general use; and the Committee have been requested to certify as to the extent in which they may be employed in the vicinity of dynamos without deterioration of their time-keeping properties.

Professor Rücker kindly undertook to arrange to conduct a series of experiments with the dynamos at South Kensington, and two students of the Royal College of Science, Messrs. Edser and Stansfield, have already submitted a preliminary report to the Committee upon the nature and extent of the influence in the magnetic field in the neighbourhood of a dynamo. The experiments are still in progress.

VII. Miscellaneous.

Lens Testing.—In the preliminary operations necessary to conduct the satisfactory examination of photographic lenses, Major L. Darwin, late R.E., has been associated with Captain Abney, and, in accordance with his suggestions, a special camera, capable of working with lenses of 4 inches aperture and 30 inches focal length, has been constructed by Mr. Meagher, and fitted up at the Observatory. A

photometer, on Abney's principle, 13 feet long, has also been fitted for use in the testing operations.

A detailed account of the apparatus and methods employed is in course of preparation by Major Darwin for publication. Meanwhile circulars, respecting the proposed scheme of examination and preliminary certificates, have been printed, and 200 distributed amongst the leading opticians, manufacturers, and secretaries of all the best known photographic societies, both at home and abroad; to call their attention to the intended plan of examination.

A fire- and burglar-proof safe has been purchased and fitted up for the reception and safe custody of the lenses.

Prepared photographic paper has been procured, and supplied to the Observatories at Aberdeen, Lisbon, Mauritius, Oxford, St. Petersburg, Stonyhurst, as well as to the Meteorological Office for Batavia, Fort William, and Valencia.

Other photographic material supplied to Observatories includes developing dishes for Colába and Oxford, as well as a camera and requisite fittings for cloud and lightning photography for Mauritius.

Anemograph sheets have been sent to Coimbra, Hong Kong, and Mauritius, and blank forms for entry of magnetic observations to Padre Denza and Professor Rücker.

Library.—During the year the library has received as presents the publications of—

44 Scientific Societies and Institutions of Great Britain and Ireland, and

114 Foreign and Colonial Scientific Establishments, as well as of numerous private individuals.

House, Enclosure, &c.—A new stove has been obtained, and fitted up in the clinical testing office to replace the one previously fitted, now worn out.

Stone blocks have been laid down on the surface of the lawn, to ensure the erection of the tripods for supporting magnetometers used for occasional observation, approximately in the proper meridian.

The roofs of the Magnetic Observatory, and of the Experimental House have been newly covered with felt, and freshly tarred.

With the view of the protection of the building against fire, and also for sanitary purposes, the Committee have made application to H.M. Office of Works and Public Buildings to have the Observatory connected with the water mains of the Corporation of Richmond, but, owing to the expense of laying down the necessary pipe, the Office has as yet been unwilling to accede to the Committee's request; nor have H.M. Commissioners provided a gas engine for pumping from the wells, capable of furnishing a sufficient quantity of water from the local springs, which they have suggested as an equivalent.

The Committee have also represented to the Office of Woods and Forests, the inconvenience to which they are at present subjected by the greatly increased cattle traffic through the existing entrance to the Old Deer Park, since the change of tenants of the park; H.M. Commissioners have for some time had under consideration the provision of a new entrance to the road leading to the Observatory, which shall avoid the passage through the two unfenced cattle lairs, at Clarence Street, Richmond, with their attendant inconvenience and danger; but no change has, up to the present, been made.

In order to provide additional space for the accommodation of the growing work at the Observatory, the Committee have obtained plans from H.M. Office of Works for the erection of two rooms on the roof of the thermometer testing room in the present west wing. They propose to proceed with building operations during next summer.

The Librarian is still engaged in the preparation of a card catalogue of the library, and has now completed over 1,700 cards, which contain the titles, &c., of all works received by the Committee during the past nine years, together with those of a like title which had been received previously.

The publications not yet catalogued formed part of Sir E. Sabine's Magnetic Office collection, and are chiefly excerpts from foreign publications and reports.

Workshop.—The machine tools procured for the use of the Kew Observatory by grants from the Government Grant Fund or the Donation Fund have been duly kept in order.

Mr. T. Fuller, the former lessee of the Old Deer Park, having resigned his tenancy, the Committee have addressed the First Commissioner of Woods and Forests, and he has decided that in future the land attached to the Observatory shall be let direct from the Crown to the Committee, without the intervention of the park tenant.

Exhibition of Instruments.—Several instruments were shown by the Committee at the twelfth annual exhibition of the Royal Meteorological Society, which was composed of Rain Gauges, Evaporation Gauges, &c.

Registration of the Committee under the Companies Act.—The Committee have come to the conclusion that it would be of advantage to them, in the transaction of their business, to obtain registration under Section 23 of the Companies Act, 1867; and they have obtained the sanction of the President and Council to their making application to the Board of Trade for this purpose. The matter is still under consideration.

PERSONAL ESTABLISHMENT.

The staff employed is as follows:—

G. M. Whipple, B.Sc., Superintendent.
T. W. Baker, Chief Assistant.
H. McLaughlin, Librarian.
E. G. Constable, Observations and Rating.
W. Hugo, Verification Department.
J. Foster " "
T. Gunter " "
W. J. Boxall, and nine other Assistants.

(Signed) FRANCIS GALTON,
Chairman.

March 11*th*, 1892.

List of Instruments, Apparatus, &c., the Property of the Kew Committee, at the present date out of the custody of the Superintendent, on Loan.

To whom lent.	Articles.	Date of loan.
G. J. Symons, F.R.S.	Portable Transit Instrument	1869
The Science and Art Department, South Kensington.	The articles specified in the list in the Annual Report for 1876, with the exception of the Photo-Heliograph, Pendulum Apparatus, Dip-Circle, Unifilar, and Hodgkinson's Actinometer.	1876
Lieutenant A. Gordon, R.N.	Unifilar Magnetometer by Jones, No. 102, complete, with three Magnets and Deflection Bar. Dip-Circle, by Barrow, one Pair of Needles, and Magnetizing Bars. One Bifilar Magnetometer. One Declinometer. Two Tripod Stands.	1883
Professor W. Grylls Adams, F.R.S.	Unifilar Magnetometer, by Jones, No. 101, complete. Pair 9-inch Dip-Needles with Bar Magnets ...	1883 1887
Professor O. J. Lodge, F.R.S.	Unifilar Magnetometer, by Jones, No. 106, complete. Barrow Dip-Circle, No. 23, with two Needles, and Magnetizing Bars. Tripod Stand.	1883
Captain W. de W. Abney, F.R.S.	Mason's Hygrometer, by Jones	1885
Prof. T. E. Thorpe, F.R.S.	Tripod Stand............................	1886
Lord Rayleigh, F.R.S.	Standard Barometer (Adie, No. 655)	1885
Mr. J. E. Cullum ...	Altazimuth Instrument, by Robinson, C. 42 ..	1891
Mr. C. Eldridge	Chain Anemometer	1890

Kew Observatory. *Account of Receipts and Payments for the fourteen months ending December 31st,* 1891.

Dr. **RECEIPTS.**

	£ s. d.	£ s. d.
To Balance from Year 1889–90		659 2 10
Royal Society (Gassiot Trust)		487 10 0
Meteorological Council:—		
Allowance		400 0 0
Postages, &c.		6 14 4
Researches		106 13 7
Tests:—		
Verifications	1153 14 11	
Rating	476 2 10	
		1628 17 9
Commissions executed for Colonial and Foreign Institutions, &c.		623 4 8
		£3912 3 2

PAYMENTS. *Cr.*

		£ s. d.	£ s. d.
By Administration:—			
Superintendent		466 13 4	
Salaries		245 19 6	
Rent, Fuel, and Lighting		61 0 8	
Attendance and cleaning of Building, Repairs, Insurance, Porterages, &c.		229 9 11	
Furniture and Fittings		8 13 3	
			1011 16 8
Normal Observatory:—			
Salaries—Observations, Tabulations, &c.		331 9 6	
Incidental Expenses—Instruments, Postages, &c.		50 19 0	
			382 8 6
Researches:—			
Salaries—Observations, Reductions, &c.		260 7 0	
Incidental Expenses—Instruments, Postages, &c.		28 3 8	
			288 10 8
Tests:—			
Salaries		1035 6 2	
Incidental Expenses—Instruments, Porterages, Printing, &c.		265 12 2	
			1300 18 4
Commissions executed for Colonial and Foreign Institutions, &c.			548 2 2
Cash in Bank of England	£290 1 1		
Cheque outstanding	5 5 0		
Balance:—			
Cash at Bank of England		284 16 1	
" London and County Bank		80 3 0	
" Observatory		15 7 9	
			380 6 10
			£3912 3 2

February 4, 1892. Examined and compared with the vouchers, and found correct. (Signed) ROBERT H. SCOTT, *Auditor.*

ESTIMATED ASSETS.

	£	s.	d.	£	s.	d.
By Balance as per Statement				380	6	10
Payments:—						
Meteorological Council—Allowance, Postage, &c.	103	3	11			
Test Fees	367	15	0			
Commissions	14	0	0			
				484	18	11
Stock:—						
Blank Forms and Certificates	44	10	10			
Standard Thermometers	89	0	0			
				133	10	10
				£998	16	7

February 10, 1892.

ESTIMATED LIABILITIES.

	£	s.	d.
To Administration accounts—Gas, Repairs, and Contingencies	40	18	6
Observatory accounts—A.G.B. Paper, Chemicals, &c.	6	11	7
Tests accounts—Fittings, Printing, Stationery, &c.	45	16	10
Unspent Balance of Pendulum Account	117	1	7
Commissions	32	15	3
General Balance	755	12	10
	£998	16	7

(Signed) G. M. WHIPPLE,
Superintendent.

Comparison of Net Expenditure for the 14 months ending December 1890, and for the same period ending December 1891.

Net expenditure.	1889—1890.	1890—1891.	Increase.	Decrease.
Administration—	£ s. d.	£ s. d.	£ s. d.	£ s. d.
Superintendent......	466 13 4	466 13 4		
Office..............	244 12 9	245 19 6	1 6 9	
Rent, fuel, and lighting..............	74 4 1	61 0 8	..	13 3 5
Alterations to premises, attendance and contingencies......	306 8 3	238 3 2	..	68 5 1
Normal Observatory—				
Salaries............	320 8 7	331 9 6	11 0 11	
Incidental expenses..	34 6 1	50 19 0	16 12 11	
Researches—				
Salaries............	233 9 9	260 7 0	26 17 3	
Incidental expenses..	42 4 10	28 3 8	..	14 1 2
Tests—				
Salaries............	1,031 1 8	1,035 6 2	4 4 6	
Incidental expenses..	167 18 3	265 12 2	97 13 11	
			157 16 3	95 9 8
			95 9 8	
	2,921 7 7	2,983 14 2	62 6 7	

APPENDIX I.

MAGNETICAL OBSERVATIONS,

Made at the Kew Observatory, Richmond, Lat. 51° 28′ 6″ N. and Long. $0^h\ 1^m\ 15^s{\cdot}1$ W., height 34 feet above mean sea-level, for the year 1891.

The results given in the following tables are deduced from the magnetograph curves which have been standardised by observations of deflection and vibration. These were made with the Collimator Magnet K.C. I. and the Declinometer Magnet marked K.O. 90 in the 9-inch Unifilar Magnetometer by Jones.

The Inclination was observed with the Inclinometer by Barrow, No. 33, and needles 1 and 2, which are $3\frac{1}{2}$ inches in length.

The Declination and Force values given in Tables I to VIII are prepared in accordance with the suggestions made in the fifth report of the Committee of the British Association on comparing and reducing Magnetic Observations.

The following is a list of the days during the year 1891 which were selected by the Astronomer Royal, as suitable for the determination of the magnetic diurnal variations, and which have been employed in the preparation of the magnetic tables.

January	4, 6, 27, 30, 31.
February	4, 8, 18, 21, 22.
March	8, 11, 19, 20, 29.
April..............	5, 15, 19, 25, 30.
May	1, 8, 21, 25, 31.
June	1, 9, 17, 22, 30.
July	2, 8, 12, 26, 31.
August............	5, 10, 18, 22, 27.
September	5, 6, 18, 19, 25.
October...........	6, 15, 16, 17, 22.
November..........	2, 8, 12, 19, 30.
December..........	3, 6, 18, 26, 29.

Table I.—Hourly Means of Declination at the Kew Observatory, Richmond, as (17° + West). Month during

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
Winter.												
1891. Months.	′	′	′	′	′	′	′	′	′	′	′	′
Jan. ..	44·9	45·1	45·5	45·8	45·7	45·4	45·1	44·7	44·5	44·2	45·6	46·7
Feb. ..	44·4	43·8	43·6	43·9	44·0	44·1	43·7	43·6	43·6	43·7	44·7	46·1
March .	42·8	42·8	43·2	43·2	42·7	42·6	42·2	41·5	40·4	41·4	43·2	46·2
Oct. ..	39·2	39·2	39·2	39·9	39·4	39·3	39·1	38·1	36·7	36·6	38·1	41·5
Nov. ..	39·2	40·2	39·9	40·2	40·4	40·3	40·3	39·9	39·2	39·3	40·3	43·0
Dec. ..	39·5	40·1	40·3	40·6	40·8	40·6	40·2	40·1	39·8	39·6	40·7	42·1
Mean	41·7	41·9	42·0	42·3	42·2	42·1	41·8	41·3	40·7	40·8	42·1	44·3
Summer.												
	′	′	′	′	′	′	′	′	′	′	′	′
April ..	42·5	42·1	42·0	41·9	41·3	41·3	40·9	39·6	38 9	40·2	42·2	44·9
May ..	40·4	39·4	39·1	39·4	38·7	37·8	37·1	36·8	37·3	39·1	42·2	45·4
June ..	40·8	40·5	40·4	40·3	39·7	38·7	37·5	36·4	36·5	37·6	40·2	42·9
July ..	39·7	39·6	39·6	39·4	38·6	36·8	36·1	35·9	35·7	36·1	38·4	41·2
Aug. ..	39·3	39·1	38·8	38·3	38·4	37·7	36·9	36·1	36·4	37·8	40·5	43·6
Sept. ..	39·5	39·5	39·1	38·7	38·1	38·0	37·5	36·8	36·6	38·1	41·3	44·9
Mean.	40·4	40·0	39·8	39·7	39·1	38·4	37·7	36·9	36·9	38·2	40·8	43·8

Table II.—Solar Diurnal Range of the Kew

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
Summer Mean.												
	′ −0·7	′ −1·1	′ −1·3	′ −1·4	′ −2·0	′ −2·7	′ −3·4	′ −4·2	′ −4·2	′ −2·9	′ −0·3	′ +2·7
Winter Mean.												
	′ −1·1	′ −0·9	′ −0·8	′ −0·5	′ −0·6	′ −0·7	′ −1·0	′ −1·5	′ −2·1	′ −2·0	′ −0·7	′ +1·5
Annual Mean.												
	′ −0·9	′ −1·0	′ −1·1	′ −1·0	′ −1·3	′ −1·7	′ −2·2	′ −2·9	′ −3·2	′ −2·5	′ −0·5	′ +2·1

NOTE.—When the sign is + the magnet

determined from the Magnetograph Curves on Five selected quiet Days in ea
the Year 1891.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid
Winter.												
′	′	′	′	′	′	′	′	′	′	′	′	′
47·9	48·4	47·7	46·6	46·3	46·0	45·8	45·7	45·5	45·1	44·7	44·7	44·9
47·5	47·7	47·4	46·6	45·6	45·3	45·3	44·9	44·7	44·7	44·2	44·1	44·4
48·5	49·3	48·7	47·6	44·9	43·7	43·4	43·2	43·5	43·2	42·8	42·8	42·8
44·5	45·9	45·2	43·6	41·8	41·2	40·6	39·9	39·5	39·4	39·1	39·1	39·2
45·0	45·8	45·3	44·2	42·8	41·7	41·4	41·1	40·5	40·1	39·5	39·6	39·2
42·5	43·0	43·2	42·5	42·3	41·1	40·6	40·1	39·9	39·2	39·1	39·2	39·5
46·0	46·7	46·3	45·2	44·0	43·2	42·9	42·5	42·3	42·0	41·6	41·6	41·7
Summer.												
′	′	′	′	′	′	′	′	′	′	′	′	′
47·1	48·2	47·0	45·6	44·1	43·1	42·6	41·7	42·3	42·6	42·3	42·4	42·5
47·6	48·4	47·3	45·6	43·1	41·9	41·0	40·8	40·3	40·9	40·9	41·2	40·4
45·6	46·8	46·8	45·8	43·8	42·5	41·3	41·2	41·5	41·6	41·6	41·3	40·8
44·0	46·1	46·6	45·7	43·6	41·7	40·3	39·6	39·8	39·7	39·8	39·9	39·7
46·1	46·4	45·7	44·1	41·8	40·2	39·3	39·6	40·0	39·9	39·4	39·2	39·3
46·8	46·8	45·5	43·4	41·7	40·8	40·0	39·9	40·0	40·1	40·0	39·8	39·5
46·2	47·1	46·5	45·0	43·0	41·7	40·8	40·5	40·6	40·8	40·7	40·6	40·4

Declination as derived from Table I.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
Summer Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+5·1	+6·0	+5·4	+3·9	+1·9	+0·6	−0·3	−0·6	−0·5	−0·3	−0·4	−0·5	−0·7
Winter Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+3·2	+3·9	+3·5	+2·4	+1·2	+0·4	+0·1	−0·3	−0·5	−0·8	−1·2	−1·2	−1·1
Annual Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+4·2	+5·0	+4·5	+3·2	+1·6	+0·5	−0·1	−0·4	−0·5	−0·6	−0·8	−0·9	−0·9

points to the west of its mean position.

Table III.—Hourly Means of the Horizontal Force at the Kew Observatory,
0·18000 + (C.G.S. units). Temperature) on Five selected quiet

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
						Winter.						
1891. Months.												
Jan. ..	184	182	183	185	188	189	189	190	187	180	178	174
Feb. ..	187	187	185	186	186	188	190	192	189	183	179	176
March .	192	190	189	188	189	190	190	187	179	175	168	172
Oct. ..	206	201	200	202	201	202	203	201	195	182	171	170
Nov. ..	190	189	189	190	192	192	194	190	185	175	166	168
Dec. ..	199	202	203	204	205	207	210	212	210	205	200	197
Mean.	193	192	192	193	194	195	196	195	191	183	177	176
						Summer.						
April ..	196	195	193	194	191	190	190	187	179	173	168	169
May ..	200	195	194	194	191	188	185	180	175	169	169	175
June ..	201	199	198	198	199	196	191	185	178	171	170	170
July ..	202	204	202	201	199	198	194	189	183	175	171	174
Aug. ..	212	209	206	208	208	204	201	195	184	177	173	182
Sept. ..	204	200	200	197	197	196	193	186	177	169	164	170
Mean ..	203	200	199	199	198	195	192	187	179	172	169	173

(C.G.S. units.) Table IV.—Diurnal Range of the Kew

Hours.	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
						Summer mean.						
	+ ·00008	+ ·00005	+ ·00004	+ ·00004	+ ·00003	·00000	− ·00003	− ·00008	− ·00016	− ·00023	− ·00026	− ·00022
						Winter mean.						
	+ ·00003	+ ·00002	+ ·00002	+ ·00003	+ ·00004	+ ·00005	+ ·00006	+ ·00005	+ ·00001	− ·00007	− ·00013	− ·00014
						Annual mean.						
	+ ·00005	+ ·00003	+ ·00003	+ ·00003	+ ·00003	+ ·00002	+ ·00001	− ·00002	− ·00007	− 00015	− ·00019	− ·00018

NOTE.—When the sign is + the

Richmond, as determined from the Magnetograph Curves (corrected for Days in each Month during the Year 1891.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
						Winter.						
174	179	184	186	186	188	189	186	185	184	184	183	184
177	179	185	187	186	186	185	186	187	188	187	186	187
176	183	186	190	187	187	188	193	192	193	192	192	192
174	184	192	195	198	202	205	206	209	208	207	207	206
171	173	177	184	186	188	189	194	196	191	191	191	190
197	200	201	202	200	203	206	207	205	203	204	201	199
178	183	188	191	191	192	194	195	196	195	194	193	193
						Summer.						
174	180	185	190	192	193	196	196	197	197	196	198	196
181	187	192	199	202	205	208	208	205	205	204	201	200
179	186	193	200	204	209	211	211	208	205	202	203	201
179	190	196	204	209	213	213	212	209	206	204	203	202
191	200	205	210	212	216	218	219	218	218	214	214	212
184	193	199	197	196	198	199	206	205	204	203	203	204
181	189	195	200	203	206	208	209	207	206	204	204	203

Horizontal Force as deduced from Table III.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
						Summer mean.						
− ·00014	− ·00006	·00000	+ ·00005	+ ·00008	+ ·00011	+ ·00013	+ ·00014	+ ·00012	+ ·00011	+ ·00009	+ ·00009	+ ·0000
						Winter mean.						
− ·00012	− ·00007	− ·00002	+ ·00001	+ ·00001	+ ·00002	+ ·00004	+ ·00005	+ ·00006	+ ·00005	+ ·00004	+ ·00003	+ ·0000
						Annual mean.						
− ·00013	− ·00006	− ·00001	+ ·00003	+ ·00004	+ ·00006	+ ·00008	+ ·00010	+ ·00009	+ ·00008	+ ·00006	+ ·00006	+ ·0000

reading is above the mean.

Table V.—Hourly Means of the Vertical Force (corrected for Temperature) at th
the Five selected quiet Days in eac

0·43000 + (C.G.S. units).

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
Winter.												
1891. Months.												
Jan. ..	958	967	967	968	967	967	968	967	966	966	967	966
Feb. ..	961	959	959	959	959	959	959	960	960	961	960	960
March .	971	964	964	965	966	967	968	970	970	967	961	956
Oct. ..	952	949	949	949	949	949	949	950	950	948	941	939
Nov. ..	944	945	945	945	945	945	945	946	946	945	943	942
Dec. ..	939	944	944	944	943	942	941	941	939	938	937	937
Mean ..	954	955	955	955	955	955	955	956	955	954	952	950
Summer.												
April ..	965	976	975	974	975	975	977	979	978	971	965	957
May ..	963	964	964	965	968	972	971	970	968	962	956	951
June ..	974	979	979	981	983	984	984	984	979	973	969	964
July ..	969	974	973	974	976	978	978	979	979	975	969	960
Aug. ..	973	974	973	974	974	977	977	977	975	972	966	962
Sept. ..	961	965	965	965	966	968	968	969	967	963	957	956
Mean ..	968	972	972	972	974	976	976	976	974	969	964	958

Table VI.—Diurnal Range of the Kew

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
Summer mean.												
	− ·0002	+ ·00002	+ ·00002	+ ·00002	+ ·00004	+ ·00006	+ ·00006	+ ·00006	+ ·00004	− ·00001	− ·00006	− ·00012
Winter mean.												
	− ·00001	·00000	·00000	·00000	·00000	·00000	·00000	+ ·00001	·00000	− ·00001	− ·00003	− ·00005
Annual mean.												
	− ·0002	+ ·00001	+ ·00001	+ ·00001	+ ·00002	+ ·00003	+ ·00003	+ ·00003	+ ·00002	− ·00001	− ·00005	− ·00009

NOTE.—When the sign is + the

Kew Observatory, Richmond, as determined from the Magnetograph Curves on
Month during the Year 1891.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid
						Winter.						
966	967	969	969	966	967	965	965	963	961	961	959	95
962	962	964	966	966	963	962	962	961	961	960	959	96
955	961	966	972	976	977	976	975	974	973	972	971	97
940	941	946	952	954	955	954	954	952	953	952	952	95
942	944	947	949	948	947	947	945	944	944	945	944	94
937	937	940	941	942	941	942	941	941	940	939	939	93
950	952	955	958	959	958	958	957	956	955	955	954	95
						Summer.						
956	959	965	971	973	974	976	975	972	970	969	967	96
949	955	962	970	973	977	980	978	976	971	967	964	96
962	964	969	972	976	982	982	981	978	976	975	974	97
956	959	964	967	971	976	976	975	973	972	970	970	96
962	964	969	976	978	979	979	977	974	973	972	973	97
954	956	959	964	966	966	966	966	964	963	962	961	96
956	959	965	970	973	976	976	975	973	971	969	968	96

Vertical Force as deduced from Table V.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid
						Summer mean.						
− ·00014	− ·00011	− ·00005	·00000	+ ·00003	+ ·00006	+ ·00006	+ ·00005	+ ·00003	+ ·00001	− ·00001	− ·00002	− ·000
						Winter mean.						
− ·00005	− ·00003	·00000	+ ·00003	+ ·00004	+ ·00003	+ ·00003	+ ·00002	+ ·00001	·00000	·00000	− ·00001	− ·000
						Annual mean.						
− ·00009	− ·00007	− ·00003	+ ·00001	+ ·00003	+ ·00005	+ ·00005	+ ·00004	+ ·00002	+ ·00001	·00000	− ·00002	− ·00

reading is above the mean.

Table VII.—Hourly Means of the Inclination at the Kew Observatory
Five selected quie

67° +

Hours.	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
						Winter.						
1891. Months.	′	′	′	′	′	′	′	′	′	′	′	′
Jan....	31·6	32·0	31·9	31·8	31·6	31·5	31·6	31·5	31·6	32·1	32·3	32·5
Feb....	31·5	31·4	31·6	31·5	31·5	31·4	31·2	31·1	31·3	31·8	32·0	32·2
March.	31·4	31·4	31·5	31·5	31·5	31·5	31·5	31·7	32·3	32·5	32·8	32·4
Oct....	30·0	30·2	30·3	30·2	30·2	30·2	30·1	30·3	30·7	31·5	32·0	32 0
Nov. ..	30·8	30·9	30·9	30·9	30·7	30·7	30·6	30·9	31·2	31·9	32·4	32·2
Dec....	30·1	30·0	30·0	29·9	29·8	29·6	29·4	29·3	29·4	29·7	30·0	30·2
Mean.	30·9	31·0	31·0	31·0	30·9	30·8	30·7	30·8	31·1	31·6	31·9	31·9
						Summer.						
April..	31·0	31·4	31·5	31·4	31·6	31·7	31·7	32·0	32·5	32·7	32·9	32·6
May...	30·7	31·0	31·1	31·1	31·4	31·7	31·9	32·2	32·5	32·7	32·6	32·0
June ..	30·9	31·2	31·3	31·3	31·3	31·5	31·9	32·3	32·6	32·9	32·8	32·7
July...	30·7	30·7	30·8	30·9	31·1	31·2	31·5	31·9	32·3	32·7	32·8	32·3
Aug. ..	30·2	30·4	30·6	30·5	30·5	30·8	31·0	31·4	32·1	32·5	32·6	31·9
Sept. ..	30·4	30·7	30·7	30·9	31·0	31·1	31·3	31·8	32·3	32·8	32·9	32·5
Mean.	30·7	30·9	31·0	31·0	31·2	31·3	31·6	31·9	32·4	32·7	32·8	32·3

Table VIII.—Diurnal Range of the

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
						Summer Mean.						
	′ −0·5	′ −0·3	′ −0·2	′ −0·2	′ 0·0	′ +0·1	′ +0·4	′ +0·7	′ +1·2	′ +1·5	′ +1·6	′ +1·1
						Winter Mean.						
	′ −0·2	′ −0·1	′ −0·1	′ −0·1	′ −0·2	′ −0·3	′ −0·4	′ −0·3	′ 0·0	′ +0·5	′ +0·8	′ +0·8
						Annual Mean.						
	′ −0·3	′ −0·2	′ −0·2	′ −0·2	′ −0·1	′ −0·1	′ 0·0	′ +0·2	′ +0·6	′ +1·0	′ +1·2	′ +1·0

Note.—When the sign is +

calculated from the Horizontal and Vertical Forces derived from the Days in each Month.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid
Winter.												
′	′	′	′	′	′	′	′	′	′	′	′	′
32·5	32·2	31·9	31·8	31·7	31·6	31·5	31·7	31·7	31·7	31·7	31·7	31·6
32·2	32·1	31·7	31·6	31·7	31·6	31·7	31·6	31·5	31·4	31·5	31·5	31·5
32·1	31·8	31·7	31·6	31·9	31·9	31·9	31·5	31·5	31·4	31·5	31·4	31·4
31·8	31·1	30·7	30·7	30·6	30·3	30·1	30·0	29·8	29·9	29·9	29·9	30·0
32·0	32·0	31·8	31·4	31·2	31·0	31·0	30·6	30·4	30·8	30·8	30·7	30·8
30·2	30·0	30·0	29·9	30·1	29·9	29·7	29·6	29·7	29·8	29·8	30·0	30·1
31·8	31·5	31·3	31·2	31·2	31·1	31·0	30·8	30·8	30·8	30·9	30·9	30·9
Summer.												
32·2	31·9	31·7	31·6	31·5	31·5	31·3	31·3	31·1	31·1	31·1	30·9	31·0
31·6	31·3	31·2	30·9	30·8	30·7	30·6	30·6	30·7	30·6	30·5	30·6	30·7
32·1	31·6	31·3	30·9	30·8	30·6	30·5	30·5	30·6	30·7	30·9	30·8	30·9
31·9	31·2	31·0	30·5	30·3	30·2	30·2	30·2	30·4	30·5	30·6	30·7	30·7
31·3	30·7	30·5	30·4	30·3	30·1	29·9	29·8	29·8	29·8	30·0	30·0	30·2
31·5	31·0	30·6	30·9	31·0	30·9	30·8	30·4	30·4	30·4	30·5	30·4	30·4
31·8	31·3	31·1	30·9	30·8	30·7	30·6	30·5	30·5	30·5	30·6	30·6	30·7

Inclination as deduced from Table VII.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
Summer Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+0·6	+0·1	−0·1	−0·3	−0·4	−0·5	−0·6	−0·7	−0·7	−0·7	−0·6	−0·6	−0·5
Winter Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+0·7	+0·4	+0·2	+0·1	+0·1	0·0	−0·1	−0·3	−0·3	−0·3	−0·2	−0·2	−0·2
Annual Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+0·7	+0·3	+0·1	−0·1	−0·2	−0·3	−0·4	−0·5	−0·5	−0·5	−0·4	−0·4	−0·3

the reading is above the mean.

APPENDIX II.—Table I.

Mean Monthly Results of Temperature and Pressure for Kew Observatory.
1891.

Months.	Thermometer.								Barometer.*					Mean vapour-tension.
	Mean.	Means of—			Absolute Extremes.				Mean.	Absolute Extremes.				
		Max.	Min.	Max. and Min.	Max.	Date.	Min.	Date.		Max.	Date.	Min.	Date.	
1891.	°	°	°	°	°	d. h.	°	d. h.	ins.	ins.	d. h.	ins.	d. h.	in.
Jan.….	34·3	38·5	29·5	34·0	51·4	31 3 P.M.	13·3	11 2 A.M.	30·155	30·737	11 10 A.M.	29·402	20 10 P.M.	·165
Feb. …	37·8	44·4	32·0	38·2	57·1	28 2 „	25·2	26 6 „	30·473	30·739	4 10 P.M.	29·882	1 2 A.M.	·194
March..	40·4	46·3	35·0	40·7	55·9	2 2 „	23·9	12 7 „	29·830	30·449	4 1 A.M.	29·250	11 4 „	·187
April…	44·1	51·6	37·2	44·4	62·4	28 2 „	27·0	1 6 „	29·981	30·312	20 11 „	29·547	5 4 P.M.	·210
May …	50·0	58·1	42·6	50·4	75·9	13 3 „	32·2	17 4 „	29·795	30·246	12 8 „	29·284	18 8 A.M.	†
June…	59·5	68·1	51·7	59·9	75·9	18 3 „	42·3	10 4 „	30·023	30·438	12 11 P.M.	29·732	4 5 P.M.	†
July …	59·7	67·8	51·7	59·8	78·8	17 3 „	45·7	28 4 „	29·943	30·305	13 Midt.	29·587	29 6 „	·391
Aug.…	58·5	65·6	51·9	58·8	73·6	14 3 „	42·5	30 5 „	29·825	30·165	8 9 A.M.	29·182	21 5 A.M.	·390
Sept.…	57·9	66·3	49·8	58·1	78·5	13 3 „	42·4	24 6 „	30·015	30·352	16 {9 „ / 10 „}	29·445	1 3 „	·389
Oct.….	50·7	57·1	44·1	50·6	64·3	9 11 A.M.	30·9	31 6 „	29·782	30·684	31 Midt.	29·071	13 10 P.M.	·312
Nov. …	43·3	47·7	37·8	42·8	55·8	19 2 P.M.	27·9	28 0·21 „	29·858	30·639	5 10 A.M.	28·466	11 11 A.M.	·246
Dec. …	40 8	45·7	35·2	40·5	56·3	5 2 ,	18·5	22 0·28 „	29·993	30·724	19 10 „	29·131	13 Noon	·219
Yearly Means	48·1	54·8	41·5	48·2	..	….	..	….	29·973	..	….	..	….	·270‡

This Table is compiled from "Hourly Means," vol. 1891, of the Meteorological Office.

* Reduced to 32° at M.S.L. † Wet-bulb instrument out of action. ‡ Mean for 10 months only.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall.*			Weather. Number of days on which were registered							Wind.† Number of days on which it was								
		Total.	Maximum	Date.	Rain ‡	Snow	Hail.	Thunderstorms.	Clear sky.	Overcast sky.	Gales.	N.	N.E	E.	S.E.	S	S.W	W.	N.W.	Calm.
1891.		in	in.																	
January.....	6·4	1·605	0·435	30	11	3	1	..	6	12	..	7	1	4	2	3	8	3	3	5
			(Dew)	15–17, 18–23																
February....	4·8	0·090	0·010	24	..	..	..	..	10	8	..	4	2	9	1	2	7	3	..	14
March......	7·3	1·320	0·460	7	17	9	..	..	..	14	2	5	4	2	1	2	5	7	5	1
April.......	7·3	0·995	0·435	4	9	..	1	..	1	16	1	6	7	6	2	1	4	1	3	1
May........	7·1	2·520	0·575	17	19	2	2	1	4	16	1	6	6	7	1	3	4	1	1	3
June.......	6·4	1·750	0·600	22	11	..	..	2	3	10	..	5	6	4	..	4	5	2	4	3
July........	6·8	2·735	0·805	19	19	..	..	3	2	12	..	4	3	1	..	5	9	7	3	3
August.....	7·5	4·055	1·155	20	23	..	..	3	1	18	..	3	..	..	1	4	15	6	2	3
September...	6·4	1·040	0·195	19	15	..	2	1	4	11	..	2	..	4	1	6	12	3	2	4
October.....	6·2	5·885	0·720	7	21	..	1	2	5	10	2	..	5	1	1	10	11	2	1	4
November...	7·6	1·935	0·450	10	17	..	1	..	1	16	1	4	5	1	2	5	4	5	4	7
December...	6·8	2·915	0·610	1	20	..	1	..	5	13	5	2	..	..	7	5	11	6	..	8
Totals and means.....	6·7	26·845			162	14	9	12	42	156	12	48	40	39	19	50	95	46	28	56

* Measured at 10 A.M. daily by gauge 1·75 feet above ground. † As registered by the anemograph.

‡ The number of rainy days are those on which 0·01 rain or melted snow were recorded.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Bright Sunshine.				Maximum temperature in sun's rays. (Black bulb *in vacuo*.)			Minimum temperature on the ground.			Horizontal movement of the air.*		
	Total number of hours recorded.	Mean percentage of possible sunshine.	Greatest daily record.	Date	Mean.	Highest.	Date.	Mean.	Lowest.	Date †	Average hourly velocity.	Greatest hourly velocity.	Date.
1891.	h. m.		h. m.		deg.	deg.		deg.	deg.		miles.	miles.	
January	73 54	28	6 18	19, 21	66	90	30	24	9	8–10, 11	10·8	32	20
February	61 12	22	7 6	15	66	92	11	28	20	2–26, 27	6·0	27	1
March	97 48	26	9 12	30	89	106	11	30	14	12	14·0	50	9
April	117 30	28	11 48	24	98	119	28	30	17	1	11·6	39	23
May	161 48	34	13 30	31	114	127	13, 31	38	24	17	9·8	37	1
June	194 18	39	13 18	1	125	137	5	47	35	12	10·3	31	1
July	175 24	35	14 0	2	125	136	17	46	40	28	9·0	26	6
August	138 48	31	10 54	29	122	134	14	48	40	30	10·3	32	25
September	151 54	40	10 36	8	116	127	2, 9	46	37	28	9·3	29	1
October	110 12	33	9 12	2	95	113	3	39	22	31	12·0	40	14
November	42 12	16	5 30	26, 27	72	92	12, 20	32	21	28	9·1	45	11
December	41 54	17	5 48	19	70	86	31	29	15	20	12·9	43	10
Totals and Means	1366 54	29	..	..	96	..	..	36	..	..	10·4	..	..

* As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground.

† Read at 10 A.M., and entered to same day.

Table IV.

Summary of Sun-spot Observations made at the Kew Observatory.

Months.	Days of observation.	Number of new groups enumerated.	Days apparently without spots.
1891.			
January	16	4	9
February	15	6	1
March	13	3	2
April	15	8	0
May	15	12	0
June	19	13	0
July	16	8	0
August	9	8	1
September	16	12	0
October	14	11	0
November	10	9	0
December	12	10	0
Totals for 1891	170	104	13

APPENDIX III.—Table I.

Results of Watch Trials. Performance of the 29 Watches which obtained the highest number of marks during the year.

Watch deposited by	Number of watch.	Balance spring, escapement, &c.	Mean daily rate — +Gaining. −Losing.	Mean variation of daily rate. ±	Mean change of rate for 1° F.	Difference of mean daily rate: Between pendant up and dial up	Difference of mean daily rate: Between pendant up and pendant right.	Difference of mean daily rate: Between pendant up and pendant left.	Difference of mean daily rate: Between dial up and dial down.	Difference between extreme gaining and losing rates.	Marks awarded for: Daily variation of rate.	Marks awarded for: Change of rate with change of position.	Marks awarded for: Temperature compensation.	Total Marks. 0—100.
			secs.	secs.	secs.	secs.	secs.	secs.	secs.	secs.				
Stauffer, Son, & Co., London	136862	Single overcoil, g.b., tourbillon chronometer	+0·2	0·3	0·03	−0·3	+0·2	+0·3	+0·4	3·0	34·4	39·3	17·9	91·6
Baume & Co., London	103011	Single overcoil, g.b., tourbillon chronometer	−1·2	0·25	0·04	−0·8	0·0	+0·2	−3·0	5·2	35·1	35·2	17·2	87·5
A. E. Fridlander, Coventry	52720	Single overcoil, d.r., g.b. lever	−0·8	0·4	0·04	−0·5	+0·7	−1·0	+3·7	6·2	32·4	35·4	17·6	85·4
A. E. Fridlander, Coventry	52730	Single overcoil, s.r., g.b.	+3·8	0·4	0·04	+2·2	+2·6	+4·0	+2·3	7·2	32·3	34·6	17·7	84·6
E. F. Ashley, London	04107	Single overcoil, s.r., fusee	−0·3	0·4	0·06	+1·9	+2·3	+1·0	−0·1	5·2	31·8	36·9	15·7	84·4
E. F. Ashley, London	04047	Single overcoil, s.r., fusee	+0·3	0·5	0·07	+1·0	+0·9	+0·2	+0·4	5·5	30·5	38·1	15·3	83·9
Usher & Cole, London	24890	Single overcoil, s.r., g.b.	+1·4	0·5	0·05	−0·5	+0·7	+2·6	−0·1	7·0	30·6	36·0	16·9	83·5
A. E. Fridlander, Coventry	52339	Single overcoil, d.r., g.b.	0·6	0·5	0·03	+0·1	2·7	−4·2	−1·3	6·2	30·7	34·2	18·3	83·2
T. B. Russell, Liverpool	86600	Single overcoil, s.r., g.b.	+1·1	0·7	0·01	+1·4	−0·4	+0·6	−0·6	5·2	25·8	37·8	19·4	83·0
W. Holland, Rock Ferry	8690	Single overcoil, s.r., g.b.	+3·4	0·5	0·03	+1·2	+2·1	+4·2	+1·3	6·8	29·1	35·5	18·2	82·8
Jos. White & Son, Coventry	32142	Single overcoil, d.r., g.b.	−0·9	0·5	0·08	+1·9	+1·3	+1·4	−1·6	5·3	30·1	37·4	15·0	82·5
Jos. White & Son, Coventry	31926	Single overcoil, d.r., fusee	+3·8	0·4	0·00	−0·1	−1·6	+2·3	+0·2	7·3	31·2	36·5	14·3	82·0
Rotherham & Sons, Coventry	86529	Single overcoil, s.r., g.b.	+1·0	0·5	0·04	+2·3	+2·8	+1·4	+0·8	8·5	29·0	35·4	17·5	81·9
A. E. Fridlander, Coventry	13113	Single overcoil, s.r., g.b.	−2·7	0·5	0·06	+0·8	−1·7	+2·0	+1·6	6·3	30·5	35·0	16·0	81·5
Jos. White & Son, Coventry	32256	Single overcoil, d.r., g.b.	+0·1	0·4	0·06	+0·3	3·1	−2·8	−0·4	5·0	31·2	34·3	16·0	81·3
J. Newsome & Co., Coventry	108491	Single overcoil, s.r., g.b.	−3·5	0·6	0·03	−2·0	−0·6	+0·7	+0·1	7·0	27·5	36·1	17·7	81·3
Rotherham & Sons, Coventry	88064	Single overcoil, s.r., g.b.	0·8	0·6	0·05	−1·0	+0·8	1·6	+1·4	6·0	27·2	37·0	17·0	81·2
A. E. Fridlander, Coventry	52704	Single overcoil, d.r., g.b.	−2·8	0·6	0·02	−2·6	+0·6	−2·4	+4·7	7·3	28·8	33·5	18·8	81·1
E. F. Ashley, London	03968	Single overcoil (palladium), s.r., g.b.	−0·2	0·5	0·11	−0·8	−2·1	0·9	+0·5	7·2	30·6	37·0	12·5	81·0
Rotherham & Sons, Coventry	86764	Single overcoil, s.r., g.b.	+1·9	0·6	0·05	+0·1	+3·1	−0·7	−0·6	7·2	28·7	35·6	16·7	81·0
A. E. Fridlander, Coventry	52706	Single overcoil (palladium), d.r., g.b., non-magnetic	−1·8	0·5	0·03	−1·2	−2·9	−1·4	+4·3	7·0	29·2	33·6	18·0	80·8
H. Golay, London	2001	Double overcoil, s.r., g.b.	+0·3	0·4	0·08	+3·9	+0·9	−0·3	−1·0	5·8	32·0	33·7	15·0	80·7

Table I—*continued.*

Watch deposited by	Number of watch.	Balance spring, escapement, &c.	Mean daily rate. — +Gaining. −Losing.	Mean variation of daily rate. ±	Mean change of rate for 1° F.	Difference of mean daily rate: Between pendant up and dial up.	Difference of mean daily rate: Between pendant up and pendant right.	Difference of mean daily rate: Between pendant up and pendant left.	Difference of mean daily rate: Between dial up and dial down	Difference between extreme gaining and losing rates.	Marks awarded for: Daily variation of rate.	Marks awarded for: Change of rate with change of position.	Marks awarded for: Temperature compensation	Total Marks. 0—100.
			secs.	secs.	secs.	secs.	secs.	secs	secs.	secs.				
E. F. Ashley, London	04022	Single overcoil, d.r., fusee	+0·1	0·6	0·06	+0·9	+1·0	+0·7	−2·9	5·0	28·2	36·4	16·0	80·6
E. F. Ashley, London	04028	Single overcoil, s.r., fusee	−0·7	0·5	0·09	+0·9	2·3	−1·5	−2·8	5·7	30·7	35·6	14·3	80·6
Jos. White & Son, Coventry	33438	Single overcoil, d.r., g.b.	+0·2	0·7	0·04	−1·6	−1·9	−1·4	+1·1	5·5	25·6	37·4	17·5	80·5
A. E. Fridlander, Coventry	52683	Single overcoil, d.r., g.b.	−2·4	0·4	0·07	−0·7	+1·7	−3·2	−2·0	6·5	31·5	33·6	15·3	80·4
Rotherham & Sons	86826	Single overcoil, s.r., g.b.	+0·8	0·6	0·08	+2·1	+2·9	+1·0	+0·8	6·0	28·3	35·8	16·3	80·4
A. E. Fridlander, Coventry	52686	Single overcoil, d.r., g.b.	−0·9	0·6	0·04	−1·4	+0·4	+0·1	+4·5	7·2	27·1	35·7	17·4	80·2
T. R. Russell, Liverpool	87023	Single overcoil, s.r., g.b.	−0·5	0·5	0·05	+2·8	+4·1	+2·5	−2·4	7·3	29·9	34·4	15·9	80·2

In the above List, the following abbreviations are used, viz. :—s.r. for single roller; d.r. for double roller; g.b. for going barrel.

APPENDIX III.—Table II.

Highest Records obtained by Complicated Watches during the year.

Description of watch.	Number.	Deposited by	Marks awarded for			Total marks, 0—100.
			Variation.	Position.	Temperature.	
Minute and split seconds chronograph, and minute repeater	2286	Jos. White and Son, Coventry	22·9	37·5	16·0	76·4
" " " "	2970	Baume and Co., London	27·8	31·7	10·7	70·2
Minute and seconds chronograph and minute repeater	2968	Baume and Co., London	28·1	35·3	15·1	78·5
" " " "	52733	Fridlander, Coventry	29·7	28·4	18·0	76·1
" " " "	26516	Usher and Cole, London	27·9	30·4	14·1	72·4
Minute and split seconds chronograph	86764	Rotherham and Sons, Coventry	28·7	35·6	16·7	81·0
" " "	86522	" " "	27·2	35·3	16·3	78·8
" " "	34920	S. Smith and Son, London	25·6	29·1	18·0	72·7
Minute and seconds chronograph	52683	Fridlander, Coventry	31·5	33·6	15·3	80·4
" " "	1868	G. Edward and Sons, London	26·3	36·5	15·5	78·3
" " "	60183	Antoine Frères, Besançon	27·3	31·4	19·3	78·0
Ordinary seconds chronograph	92485	Rotherham and Sons, Coventry	26·2	34·8	18·3	79·3
" " " "	1986	Golay, London	30·2	31·8	11·3	73·3
" " " "	98764	Rotherham and Sons, Coventry	22 1	32·9	16·7	71·7
Minute repeater	36693	S. Smith and Son, London	26·0	34·4	7·9	68·3
" "	52756	Fridlander, Coventry	23·7	26·8	14·7	65·2
" "	9694	W. Clarke, London	16·7	32·2	15·1	64·0

First Report to the Water Research Committee of the Royal Society, on the present State of our Knowledge concerning the Bacteriology of Water, with especial reference to the Vitality of Pathogenic Schizomycetes in Water.

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Introduction.

The interest attaching to the presence of micro-organisms in water originated principally in the proof, which has been furnished by medical men, that some zymotic diseases are communicated through drinking water. In the case of two diseases, at any rate, the evidence may be regarded as conclusive on the main point, and the communicability of Asiatic cholera and typhoid fever forms one of the cardinal principles of modern sanitary science, which year by year is becoming more widely recognised and generally accepted. The germ theory of zymotic disease, which has become more and more firmly established during each successive decade of the past half century, was naturally soon impressed into the service of those who sought to explain the empirical fact that these particular diseases are frequently communicated by water. It is significant that these views concerning the propagation of cholera and typhoid, and the importance to be, therefore, attached to drinking water in connexion with public health, are mainly English in origin, and were for many years unflinchingly preached and practised by English sanitary authorities, at a time when the germ theory of disease was a speculation, and not established as it now is. It is only necessary to read, by the light of our knowledge of to-day, the Sixth Report of the Rivers-Pollution Commissioners (1868), written more than eighteen years ago, to be convinced of the intuitive sagacity and acumen which has been displayed in this country in matters of sanitation.

The germ theory of disease having thus so early become interwoven with the consideration of potable waters, it is easy to understand with what eager interest the vigorous development in our knowledge of micro-organisms in general, and of their connexion with

disease in particular, which has taken place within the past fifteen years, has been watched by those who have had to devote much attention to the sanitary aspects of water-supply.

The subject of our particular enquiry is, therefore, a modern one, and may be regarded as having been made amenable to successful treatment by the introduction of Koch's method of gelatine-plate cultures in 1881, and subsequent improvements on the same.*

The publication by Koch of his beautiful and comparatively simple methods of bacteriological study gave an impulse to this kind of work throughout the civilised world. These methods, in spite of certain imperfections from which they never professed to be free, at once opened up the possibility of solving a number of problems connected with water-supply which had long been matters of dispute and speculation amongst hygienic authorities.

As is now well known, Koch's method of gelatine-plate culture admits of an estimate being made of the number of living germs present in a given quantity of any material. That this estimate is a very rough one, and that there are a number of different kinds of germs which it is incapable of revealing, was also known, and has been more or less admitted from the outset. Now this possibility of estimating the number of microbes in a given volume of water has been largely made use of by numerous investigators for a number of different purposes.

In the first instance, this method has been extensively employed both in this country and abroad for determining the relative richness in micro-organisms (capable of developing in gelatine) of various natural waters. On the Continent, and more especially in Germany, it was assumed that the relative numbers of microbes present in different waters afforded evidence of the extent to which they had been contaminated, and there were not wanting those who hastily set up arbitrary standards of purity based upon a most limited experience of the number of micro-organisms revealed by the gelatine test. By those who were simultaneously employing the method in this country

* Koch's first announcement of his new method was made at the meeting of the International Medical Congress in London, in August, 1881 (see 'Quart. Journ. Microsc. Sc.,' Oct., 1881, p. 650), and then published in the 'Mitth. aus d. K. Gesundheitsamte,' vol. 1, 1881 (see also 'Berl. Klin. Wochenschr.,' 1882, No. 5). It should be noted, however, that Koch's gelatine-plate method was an improved adaptation of one introduced long before by the botanists Brefeld and Klebs (see Brefeld, "Methoden zur Unters. der Pilze," 'Abh. der Phys.-Med. Gesellsch. in Würzburg,' 1874; also 'Landwirthsch. Jahrb.,' vol. 4, H. 1, and 'Unters. über Schimmelpilze,' 1881, H. 4; also Klebs, "Beitr. zur Kenntn. d. Mikrokokken," 'Arch. für exp. Pathol.,' vol. 1, 1873, and De Bary, 'Lectures on Bacteria,' Engl. ed., 1887, p. 35). A fair view of the matter is given by Hueppe, 'Die Methoden der Bakterien-Forschung,' 1885, p. 103. To Koch belongs the credit of having applied these plates to the purpose of separating the various colonies of mixtures of micro-organisms.

more caution and reserve were exercised in this respect, the results being tentatively chronicled without comment or deduction as to purity.

The prudence of this caution was soon apparent, when both in England and in Germany it was discovered in 1885 that many forms of micro-organisms could multiply to an astonishing extent in waters of great organic purity, including distilled water itself. It is obvious that this discovery at once subverted the artificial standards which had been proposed, for, although a small number of micro-organisms might frequently point to little or no contamination having taken place, a large number could only under special circumstances be viewed as conclusive evidence of proportionate contamination.

Another question was, however, being simultaneously attacked, both in this country and on the Continent, viz., the effect of various processes of purification on the bacterial life present in water. This question, which is of evident importance, could be investigated with considerable precision by means of the gelatine-plate method, in spite of its imperfections, for by applying the same mode of culture to a particular water before and after purification, it is obvious that the defects neutralise each other, and that a true differential result can be obtained. In this manner, the value of a number of filtering materials has been ascertained, and the remarkable efficiency of water-works sand-filtration for the first time established. Again, the fact that practically all surface waters exhibit a large number of microbes by the gelatine test, whilst deep well and spring waters yield very few, or in some cases none, shows how perfect, as regards removal of micro-organisms, must be the natural filtration which water undergoes in traversing great depths of porous strata in the earth.

It has also been shown that in the subsidence of solid particles often a surprisingly large proportion of the micro-organisms present in water are carried to the bottom, a matter which is of particular interest and importance in connexion with the natural purification of surface waters in rivers, lakes, and storage reservoirs, as well as in the artificial treatment of water-supplies by Clark's softening process, and other methods of precipitation. In applying the gelatine test to these various purification processes it is, however, essential, if an accurate result is to be obtained, that the water should be examined *immediately* (within a few hours) after the purification is completed, and before the number of microbes in the purified water can have multiplied naturally.

Another use to which the bacteriological methods have been applied is to the actual discovery of pathogenic microbes in water, and, in the opinion of some, this should indeed be the primary object of bacteriology in connexion with water-supply. This view appears

to us, however, too narrow, as waters are surely to be condemned for drinking purposes not only when they contain the germs of zymotic disease at the time of analysis, but in all cases when they are subject to contaminations which, like sewage, may at any time convey such germs.

Notwithstanding that great difficulties usually invest the discovery of particular pathogenic forms in the presence of large numbers of ordinary water microbes, the spirillum of Asiatic cholera was early discovered in water by Koch, whilst the bacillus of typhoid fever has more recently been on several occasions detected by various investigators in drinking waters which had been suspected of communicating this disease. It is obvious that such discoveries are rather of interest on account of their confirming the belief in the communicability of these diseases through water than of special hygienic importance in preventing outbreaks of zymotic disease.

The bacteriological methods of examination have been also employed for a purpose of far greater importance than the mere quest for pathogenic forms in particular water-supplies, viz., for direct experiments on the vitality of known pathogenic organisms in waters of different composition and under varied conditions of temperature and the like. This investigation was also begun independently and almost simultaneously both in this country and on the Continent, and although the question appears at first sight a very simple one, it is in reality surrounded with numerous pitfalls, which have, in some cases, led to very discordant results. Perhaps the greatest difficulty which attaches to this investigation consists in the very different degrees of vitality which are exhibited by one and the same organism at different periods of its life, and according to the previous treatment which the individual and its ancestors have received. On this account a number of individuals of a particular species, on being introduced into a given water, may conduct themselves quite differently from a number of individuals of the same species, but taken from a different source and having another ancestral history. This importance of individuality and pedigree, with which we are so familiar in dealing with the higher plants and animals, must also invariably be taken into consideration in connexion with these lowly forms of life.*

In spite of some conflicting results which have thus not unnaturally been obtained in these investigations, it is sufficiently evident that some pathogenic species, and notably those which form spores, are capable of retaining their vitality in ordinary potable waters for a

* Particular cases of the application of these methods to the examination of food substances, brewing and distilling waters, the materials of clothing, dyeing and other industries lie beyond our present enquiry; their importance is obvious, as is also the utility of the methods for the analyses of air, soils, &c.

long period of time, in some cases several months, whilst other forms, especially in the absence of spores, are very rapidly destroyed. In some instances actual multiplication, although but rarely on any very extensive scale, has been observed in the case of some pathogenic forms in potable water, and more frequently in badly contaminated waters and in sewage. In those cases in which pathogenic forms have been introduced into waters in their natural condition, the destruction of these forms has almost invariably been more rapid, and in some cases very much more rapid, than when the same forms were introduced into the same waters after the latter had been previously sterilised. This more rapid disappearance of the pathogenic forms in the unsterilised water is generally accounted for by assuming that they have perished in the struggle for existence with the other microbes present in the water, and which by their nature and previous history are more fitted for this aquatic existence. We are of opinion that these results with unsterilised water must be accepted with considerable caution, as the experimental difficulties involved are very great indeed, and the possibility of the comparatively few pathogenic forms being undiscovered amidst the countless swarms of water organisms must be borne in mind.

Having thus given a general survey of the present position of our knowledge concerning the bacteriology of water, we shall now proceed to enter into each of the questions involved in more detail, giving special attention to the existing literature on the several parts of our subject.

In view of the fact that the introduction of Koch's methods, in 1881, thus revolutionised the subject of hygiene, we naturally commence our review of the literature at the period just previous to and after the above date. There are, in fact, practically no investigations on the more special part of our subject anterior to this date.

Most of the more systematic work has been done on the Continent, and, at first, especially in the public laboratories of Germany, where the new methods were at once applied industriously to the examination of the bacteria in the air, the soil, food stuffs, water, &c.

A number of French enquirers also arose, though it is significant that Koch's gelatine-plate method to this day has not found much favour in one of the oldest and best of the French stations, the Observatoire de Montsouris, whence excellent work has come.* England, Italy, America, and other countries soon followed; but, although a number of exceedingly valuable memoirs on special questions relating to the general subject were published at an early date in this country and in France, it is still noticeable that most of the prolonged and systematic investigations of the bacterial life to be found in the waters

* See 'Annuaire de l'Observatoire de Montsouris,' 1877 to 1891, and Miquel, 'Manuel Pratique d'Analyse Bactériologique des Eaux,' Paris, 1891.

of towns, &c., and statistical comparisons of their behaviour at different seasons, their vitality when placed in special waters, and so on, have come from Germany. Of late years it would be difficult to say where most activity has been displayed, the establishment of the Pasteur Institute in Paris having led to the inauguration of brilliant and industrious researches into every aspect of the questions we are concerned with. Even now, however, it is especially to the reports of the public hygienic institutions of Germany that we must turn for the more technical and systematic comparative researches into the main question of our enquiry—the length of life enjoyed by specific Schizomycetes when placed in arbitrarily selected waters.

We propose to adopt the following plan of treatment in this Report:—

(1.) To give some account of what species or forms are actually known to occur in natural waters, of various kinds, whether as more or less normal and constant inhabitants of such waters, or as casual foreign intruders. Some of these are well known, others we know very little about, while yet others require careful examination before we can accept them as autonomous. We also refer to a few remarkable forms found in special kinds of water, or under peculiar conditions, only.

(2.) We propose to draw attention to some of the facts now acquired, which throw light on the questions of the relations subsisting between the bacterial contents of natural waters, such as those of rivers, springs, wells, reservoirs, &c., and the air and soil of the neighbourhood, because some of these facts are of great importance as regards the presence and supply of pathogenic germs in such waters. This leads naturally to the discussion of recent experimental investigations into the effects of certain factors of the physical environment, *e.g.*, temperature, light, oxygen, &c., in modifying the life-actions of such bacteria as we are concerned with, and so far as such effects may be expected to affect the general problems at issue.

(3.) The questions, What bacteria can live in such waters as we refer to? Can they multiply and spread in them? Can definitely chosen pathogenic species live and multiply in selected waters; and, if so, how long can they maintain themselves? These, and similar questions relating to them, will be treated in detail as fully as the literature and our own personal experience in these matters enable us to do.

This will, we think, cover the ground of the history and literature of the subject, and will clear the way for our proposals as to lines of enquiry to be entered into forthwith, and as to the methods we intend to put in practice in pursuing the question further.

§ I. *Schizomycetes actually Detected in Natural Waters.*

It has long been known that natural waters, of rivers, springs, ditches, wells, lakes, &c., contain bacteria, often in enormous quantities; and prolonged experience has shown that no kind of ordinary water, whether running or standing, is entirely devoid of these organisms;† though the rule is that the waters of rivers usually contain many more organisms than those of lakes or large reservoirs. Mountain streams are generally less rich in bacterial life, while stagnant or slowly moving waters usually contain many and often peculiar species.

Some of the best studied and longest known aquatic species are the following:—*Cladothrix dichotoma* (Cohn), *Crenothrix Kühniana* (Rabenh.), *Sphærotilus natans* (Kütz.), *Beggiatoa alba* (Vauch.), *Spirillum plicatile* (Ehrenb.), *Bacillus erythrosporus* (Cohn), *Bacterium janthinum* (Zopf), *Bacterium merismopedioides* (Zopf). There are, however, many other forms known to occur in water: *e.g.*, *Sarcina Reitenbachii* (Caspary), *S. hyalina* (Kütz.), *Spirillum serpens** (Müller), *S. tenue** (Ehrenb.), *S. undula** (Müller), *S. volutans** (Ehrenb.), *Monas vinosa** (Ehrenb.), *M. Okenii** (Ehrenb.), *M. gracilis* (Warm.), *Rhabdomonas rosea** (Cohn), *Myconostoc gregarium* (Cohn), *Spiromonas Cohnii* (Warm.), *Micrococcus crepusculum** (Ehrenb.), *M. griseus* (Warm.), *Clathrocystis roseo-persicina* (Kütz.), *Bacterium Termo** (Dujard.), *B. lineola* (Müller), *Bacillus tremulus* (Koch), *B. Ulna** (Cohn), *B. virens* (Van Tiegh.), and many others.‡

With regard to this last list, two remarks are necessary. In the first place, most of the species or forms named are characteristic of waters containing much vegetable or animal matter in a state of decay, whence they are commoner in marshes, ditches, ponds, &c., than in running streams, rivers, and wells; and, in the second, some of the so-called species are certainly not good ones, but what are usually termed "form-species," requiring further examination, and especially by means of continuous cultures. The latter applies particularly to the forms marked * in the above list; we do not accept all the others as satisfactory species, but it is difficult to say anything definite about them. One form (*Clathrocystis roseo-persicina*) has been shown by Lankester§ and Zopf|| to be very polymorphic, and it

† 'Cohn, 'Beiträge zur Biologie der Pflanzen,' from 1870; as well as in numerous reports concerning drainage from sugar factories, Magdeburg, 1886; Hirt, 'Zeitschr. f. Biologie,' 1879, vol. 15, p. 91; Eyferth, 'Die Mikroskopischen Süsswasserbewohner,' 1877; Kirchner and Blochmann, 'Die Mikroskopische Pflanzen- und Thierwelt des Süsswassers,' 1886; Hulwa, 'Biedermann's Centralbl. f. Agrikulturchemie,' 13, Pt I.

‡ See Appendix A for literature.

§ Who named it *Bacterium rubescens*.

|| Who terms it *Beggiatoa roseo-persicina*.

is at least not impossible that the forms described as *Bacterium sulphuratum*, *Merismopedia littoralis*, *M. Reitenbachii*, *Monas vinosa*, *M. Warmingii*, *M. erubescens*, *M. Okenii*, *M. gracilis*, *Spirillum violaceum*, *Rhabdomonas rosea*, &c., by different observers are merely form-phases of Lankester's species.* So long as investigators are content with recording and naming forms without cultivating them, such difficulties as the above will increase, and most of the trouble met with in the literature of bacteriology—so far as their morphology is concerned—is traceable to this error.

A still longer list of bacteria are recorded as having been occasionally met with in various waters, though most of them are not characteristic of such habitats. Among these are: *Bacillus subtilis* (Ehrenb.), *Proteus vulgaris* (Hauser), *Bacillus anthracis* (Cohn), *Spirillum choleræ-asiaticæ* (Koch), *Bacillus typhosus* (Eberth-Gaffky), *B. aquatilis sulcatus* (Weichselbaum), *B. dysentericus* (Aradas), *B. thermophilus* (Miqu.), *Micrococcus agilis* (Ali-Cohen), and others.†

Here, again, it must be remarked that many of the forms are unsatisfactory as regards their autonomy—*e.g.*, *Proteus vulgaris*, *Micrococcus agilis*—and that we find no record of the authority for *B. dysentericus*, referred to by Aradas.

We have decided to omit detailed references to numerous other forms, recorded as occurring in water, but under names which convey no specific meaning, though some of them are not necessarily bad records; others, again, are referred to by the authors in such a loose way that it is impossible to accept them until they have undergone revision. Thus, Eisenberg refers to rodlets which he dubs "*Violetter-bacillus*," "*Gasbildender-bacillus*," "*Verflüssigender-bacillus*," &c.,‡ and Perdrix, in an excellent paper§ in other respects, speaks of *Bacille amylozyme*, a form met with in the rivers of Paris. So far as it has been possible we have given these in the lists in Appendix B; but we desire to point out that the labour of hunting up the synonyms recorded for these and other imperfectly named forms is very great, and that, therefore, omissions may possibly be discovered with regard to some of them.

Among such forms we may also mention *Bacillus arborescens*, *B. liquidus*, *B. vermicularis*, *B. nubilus*, *B. ramosus*, *B. aurantiacus*, *B. viscosus*, described by one of us;|| also Meade Bolton's *Micrococcus*

* Winogradsky denies the accuracy of Zopf's views on this matter, however (see 'Beiträge z. Morph. u. Phys. der Bakt.,' 1888).

† See Appendix B for a complete list of these and all other bacteria known to occur in various waters.

‡ 'Bakteriologische Diagnostik,' Berlin, 1886. Some of these forms have since received definite names, adopted by Eisenberg in the edition of 1891. (See Appendix B.)

§ 'Ann. de l'Inst. Pasteur.' vol. 5, 1891, pp. 286—311.

|| Grace C. and Percy F. Frankland, 'Zeitschr. f. Hygiene,' vol. 6, 1889, p. 373.

aquatilis;* the *Vibrio saprophiles*, *V. aureus*, *V. flavus*, and *V. flavescens* of Weibel,† the series of nine micrococci and bacilli isolated by Adametz‡ from certain drinking waters, and a number of forms referred to by Bokorny,§ Rintaro Mori,‖ Allen-Smith,¶ Macé,** and several other workers during the last ten years.††

Enough has been said to give point to the statement that Schizomycetes of various kinds are common in ordinary waters. Some of the forms referred to are almost universally distributed, at least in Europe and America.‡‡ Others are mostly confined to foul or contaminated waters, containing decaying animal and vegetable matter,§§ &c., such as pools on moorlands, dirty ditches and canals, docks, &c.

There remains, however, a very large amount of work still to be done in connecting particular forms with particular kinds of water and with particular sources of contamination, which would enormously extend the scope and enhance the value of the bacteriological examination of water from a sanitary point of view.

Other forms, again, are not known to be characteristic of any particular class of waters, but have occurred at intervals in any‖‖ of them, suggesting that they have gained access as casual intruders. At the same time it may be noted that the systematic determination of forms in natural waters has not yet been sufficiently extensive to warrant definite and positive statements as to the habitats of the species. A shorter list, but one that is growing longer every year, includes forms which are not typically characteristic of waters at all, but are pathogenic species, which have almost certainly been introduced into the waters with foreign matters.¶¶

We have so far confined our attention to waters which are at least sometimes employed for household purposes, and are generally known as "fresh" waters, though they differ enormously in detail if we contrast those of dirty rivers with those of clear streams, or those of open moorlands and marshes with those of wells and springs, and so forth.

* 'Zeitschr. f. Hyg.,' vol. 1, 1886, pp. 76—114.

† 'Centralbl. f. Bakteriol.,' vol. 4, 1888, p. 225.

‡ 'Mitth. der Oesterr. Versuchsst. f. Brauerei u. Malzerei in Wien,' 1888, H. 1.

§ 'Arch. f. Hyg.,' vol. 8, 1888, p. 105.

‖ 'Zeitschr. f. Hyg.,' vol. 4. 1888, pp. 47—54.

¶ 'Medical News,' 1887, p. 758.

** 'Annuaire d'Hyg. et de Méd. Légale,' vol. 17, 1887, No. 4.

†† See Appendix A for further literature.

‡‡ *E.g.*, *Cladothrix dichotoma*, *Crenothrix Kühniana*, *Beggiatoa alba*, &c.

§§ *E.g.*, *Clathrocystis roseo-persicina*, *Sphærotilus natans*, *Bacterium janthinum*, *B. merismopedioides*, &c.

‖‖ *E.g.*, *Bacillus erythrosporus*, *B. subtilis*, *Micrococcus agilis*, &c.

¶¶ *E.g.*, *Bacillus anthracis*, *B. typhosus*, *Spirillum choleræ asiaticæ*, and others which give rise to disease.

But there are micro-organisms in other classes of waters, not usually employed for domestic purposes on a large scale, *e.g.*, the sea, salt marshes and springs, sulphur springs, and mineral waters of various kinds. As a rule, the bacteria of such waters are different from the above, and often appear to be more restricted in species. Thus:—

In the sea are found *Sarcina litoralis* (Oerst.); *Bacterium fusiforme* (Warm.); *B. Pflügeri* (Ludw.); *Beggiatoa mirabilis* (Cohn); *Phragmidothrix multiseptata* (Engler); *Bacterium litoreum* (Warm.); *Beggiatoa pellucida* (Cohn); *Spirochæte gigantea* (Warm.); *Spirillum volutans** (Warm.); *S. sanguineum** (Ehrenb.); *S. violaceum** (Warm.); *Monas Mülleri** (Warm.), and some others. Here, again, an enormous field exists for further research.

Spirillum Rosenbergii (Warm.) and *S. attenuatum* (Warm.), and some other imperfectly studied forms, affect brackish waters; while *Crenothrix polyspora*, *Cladothrix dichotoma*, and *Leptothrix ochracea* (Kütz.) are described as especially occurring in stagnant waters rich in iron,† and certain other species are found in sulphur springs, and even secrete sulphur in their cells:—*e.g.*, *Beggiatoa alba*, *Monas Okenii*, *Clathrocystis roseo-persicina*, *Sarcina sulphurata* (Winogr.), *Monas vinosa*, and a series of other forms.‡

Others, again, are met with in warm springs, *e.g.*, *Sphærotilus thermalis* (Kütz.), and *Sph. lacteus* (Kütz.), *Detoniella lutea* (Kütz.), *Beggiatoa arachnoidea* (Ag.), *B. alba* (Vauch.), and others.

Many of the foregoing species, and others referred to in Appendix B, p. 244, may be regarded as water-bacteria, *i.e.*, as forms more or less habituated to life in natural waters; but, as already stated, some must be looked upon as occasional intruders, introduced temporarily with foreign matters.

Owing to the importance of some of these forms in causing diseases in man and other animals, it is necessary to look with grave suspicion on waters that give evidence of their presence, and we must devote special attention to these, as most closely connected with the subject which we have in hand.

The literature concerning these pathogenic forms in water is almost daily increasing: quite recently Tils§ has found *Staphylococcus pyogenes aureus* in town water.

Passing over the strong presumptive evidence that Koch's *Spirillum choleræ asiaticæ* is an aquatic form native to the waters in Bengal, it

* All these stand much in need of further investigation.

† See Winogradsky, "Ueber Eisenbacterien," 'Bot. Zeitg.,' 1888, p. 261. *Crenothrix polyspora* is merged into *C. Kühniana* by Zopf; but see Winogradsky, 'Beitr. z. Morph. u. Phys. d. Bakt.,' 1888.

‡ See Winogradsky, "Ueber Schwefelbacterien," 'Bot. Zeitg.,' 1887, pp. 489, *et seq.*, and 'Beitr. z. Morph. u. Phys. d. Bakt.,' 1888.

§ "Bacteriologische Unters. der Freiburger Leitungswässer," 'Zeitschr. f. Hyg., 1890, vol. 9, pp. 282—322.

may be accepted that it occurs in water (tanks in India) as an occasional impurity.*

Instances of the detection of the bacillus of typhoid fever (*Bacillus typhosus*, Eberth) in waters used for domestic purposes also seem to be established;† though it should be insisted on that great difficulties still stand in the way of directly recognising this species.‡

Of forms pathogenic to the lower animals, there have been found in water the bacterium which causes septicæmia in rabbits (*Bacillus cuniculicida*, Koch), which was originally met with in the waters of the River Panke, at Berlin, according to Koch.§

Rintaro Mori has also recorded the occurrence of three pathogenic species in the water of a certain drainage-canal.|| These are (1) *Bacillus murisepticus*¶ (Koch); (2) a form which Mori names "*Kapseltragender Canal-bacillus*," resembling in some respects Friedländer's *Bacillus pneumoniæ*, but not identical with it; and (3) a form, also unidentified as yet, which the author simply records as "Canal-bacillus." These forms or species were very constant in the drainage-canal water, and were proved to be pathogenic by infecting mice and guinea-pigs with them.

* Koch, in 'Berliner Klin. Wochenschr.,' 1883–84; 'Bericht über die Thätigkeit der zur Erforschung der Cholera im Jahre 1883 entsandten Commission,' Berlin, 1887, p. 182. Nicati and Rietsch found the same spirillum during a cholera epidemic in the harbour of Marseilles ('Rev. d'Hygiène,' 1885, May 20).

† Loir, in 'Ann. d. l'Inst. Pasteur,' vol. 1, 1887, p. 488, and Cassedebat, in same Annals, vol. 4, 1890, pp. 625—640.

The typhoid bacillus (Eberth-Gaffky) was first detected in water by Moers ("Die Brunnen d. Stadt Mülheim a. Rhein vom Bakteriologischen Standpunkte aus betrachtet," 'Ergänzungshefte, Centralbl. f. allgem. Gesundheitspflege,' 2, 1886, p. 144); then by Michael in Dresden ("Typhusbacillen im Trinkwasser," 'Fortschritte d. Medizin,' 4, 1886, No. 11, p. 353); for the third time by Dreyfus-Brisac and Widal ("Epidémie de Fièvre Typhoïde, Considérations Cliniques et Recherches Bactériologiques," 'Gaz. Hebdom.,' 1886, No. 45); then, for the fourth time, by Chantemesse and Widal ("Enquête sur une Épidémie de Fièvre Typhoïde qui a régné à Pierrefonds, 1886," 'Rev. d'Hygiène,' 9, p. 116; 'Archiv. de Phys. et Pathol.,' 1887, p. 217); also by Reumer ("Zur Aetiologie d. Abdominaltyphus," 'Deutsche Mediz. Wochenschr.,' 1887, No. 28), as well as by others. For complete summary, see Jaeger, "Zur Keuntniss d. Verbreitung d. Typhus durch Contagion und Nutzwasser," 'Zeitschr. f. Hygiene,' 10, 1891, p. 197.

‡ Parietti has recently published a means for distinguishing it from the false forms. See 'Ann. de l'Instit. Pasteur,' vol. 5, 1891, p. 414.

§ 'Mittheil. aus d. Kaiserl. Gesundheitsamte,' 1881, vol. 1, p. 94.

|| "Ueber Pathogene Bacterien im Canalwasser," 'Zeitschr. f. Hygiene,' vol. 4, 1888, pp. 47—54.

¶ This had also previously been found in the water of the Panke (Gaffky-Loeffler, 'Mittheil. a. d. K. Gesundheitsamte,' 1, pp. 80 and 135).

§ II. *On some Relations between the Bacteriological Contents of Water and the Environment.*

Although the evidence on which the above statements are founded is of unequal value in different cases, it may be regarded as established that the germs of pathogenic Schizomycetes do occasionally occur in waters used for domestic purposes, and the records of medical literature fully bear out this conclusion.*

The next point for discussion is, How do these germs find their way into potable waters?

It seems capable of proof that water, as such, does not necessarily contain any bacteria at all, for if it is examined at the source of deep springs, or in the deep subterranean layers tapped by artesian well-pipes, it is found to be either wholly or practically free from organisms at or near the source. Moreover, it is an axiom that, in cases where the water-supply is drawn from rivers, there are more bacteria as we go towards the mouth, and fewer as we ascend the heights of the watershed; whilst the gain in bacteria, both as regards forms and numbers of individuals, is marked below each town or inhabited area through which the river flows.†

We now proceed to the questions, Why are the deep waters below the sub-soil free from germs? How do they become contaminated

* Especially for typhoid. See Vaughan and Novy ('Med. News,' 1888, p. 92), Charrin ('Ann. d'Hyg. Publique et de Méd. Légale, 1887, pp. 520—529), Brouardel et Chantemesse ('Ann. d. Hyg. Publ. et de Méd. Lég.,' 1887, No. 12), Hauser and Kreglinger ('Die Typhus Epid. in Triberg in den Jahren 1884 und 1885,' Berlin, 1887), and the literature already quoted.

† For more details in support of these statements, consult:—Burdon Sanderson ('Rep. of the Med. Officer of the Privy Council,' 1870 and 1872), Angus Smith ('Rep. Med. Officer Local Gov. Board,' 1884), Fol and Dunant ('Arch. des Sc. Phys. et Natur. de Genève,' 1884 and 1885), Di Vestea and Tursini ('Recherches sur les Eaux de Naples,' 1885), Cramer ('Die Wasserversorgung von Zürich,' 1885), Percy F. Frankland ('Monthly Reports to the Local Government Board on the Bacteriological Examination of the London Water Supply,' 1885—1888; also 'Journ. Soc. Chem. Industry,' 1885 and 1887; 'The Present State of our Knowledge concerning the Self-Purification of Rivers,' Internat. Congress of Hygiene and Demography, 1891), G. Bischof ("Notes on Dr. Koch's Water Test," 'Journ. Soc. Chem. Industry,' 1886), C. Fraenkel ("Unters. ü. Brunnendesinfection u. d. Keimgehalt d. Grundwassers," 'Zeitschr. f. Hyg.,' vol. 6, 1889, pp. 23—61), Koch ('Die Bekämpfung der Infections-Krankheiten: Rede zur Stiftungsfeier der Militärärztlichen Bildungsanstalten,' 1888, p. 25), Plagge and Proskauer ('Zeitschr. f. Hyg.,' vol. 2, 1887, p. 401), Wolffhügel ("Erfahrungen ü. d. Keimgehalt brauchbarer Trink- u. Nutz-Wässer," 'Arb. a. d. Kaiserl. Gesundheitsamt,' vol. 1, 1886, pp. 546—566), G. Frank ("Die Veränderung des Spree-Wassers innerhalb und unterhalb Berlin, &c.," 'Zeitschr. f. Hyg.,' vol. 3, 1888, pp. 355—403), Schlatter ('Der Einfluss des Ab-wassers der Stadt Zürich auf den Bacteriengehalt der Limmat," 'Zeitschr. f. Hyg.,' vol. 9, 1890, pp. 56—58). See also 'Ann. d. l'Instit. Pasteur,' vol. 3, 1889, pp. 559—569 and our Appendix A, *infra.*

subsequently? And, what relations subsist between the facts elucidated and the questions concerned in our enquiry?

The purity of the subterranean waters is certainly not due directly to the rain which falls on the land (and which is, of course, the original source of such waters) being devoid of germs; because, in the first place, much of this rain is already contaminated before it touches the soil, by bacteria suspended in the air, and secondly, because the instant that the rain touches any ordinary soil it becomes abundantly contaminated with micro-organisms.

So long as this water is near the surface of the soil, it forms, in fact, a medium admirably adapted for the growth and multiplication of the myriads of Schizomycetes and other organisms with which the surface soil teems.

It is this surface water flowing off the land into our rivers, open wells, &c., which so abundantly contaminates them by carrying with it, not only the microbes themselves, but also the soluble organic and mineral matters which serve them as food materials.*

As the rain water percolates into the land, however, more or less of it soon sinks to levels below those at which there is danger of its emerging as a contaminating fluid, and in this process of percolation two important changes take place. Firstly, as the water passes from the surface soil to the sub-soil, it leaves behind it both soluble and suspended matters: certain of its salts are retained in the films on the surfaces of the particles of earth, whilst other salts become dissolved in it; it is also, to a great extent, deprived of its dissolved oxygen, and a large proportion of its suspended germs are held back in the capillary interspaces.† In the subsoil, the water is in contact with Schizomycetes of quite different nature from those in the well-aërated surface soil, rich in organic and other food materials, and although these anaërobic organisms of the deeper layers are not necessarily less injurious, or otherwise, than the aërobic forms in the

* See especially two admirable reviews by Duclaux, in 'Ann. de l'Inst. Pasteur,' vol. 4, 1890, on "Le Filtrage des Eaux," pp. 41—56; and "Sur les Relations du Sol et de l'Eau qui le traverse," pp. 172—184.

† Of the extraordinary power possessed by even thin strata of suitable filtering materials of arresting microbes present in the water passing through them abundant evidence has been furnished by one of us (Percy F. Frankland, "On the Removal of Micro-organisms from Water," 'Roy. Soc. Proc.,' 1885), as well as by Hesse ("Ueber Wasserfiltration," 'Zeitschr. f. Hygiene,' 1, 1886, p. 178), Pöhl ("Ueber Filtration d. Newa-wassers," 'Centralbl. f. Bakteriologie,' 1, 1887, p. 231), Bertschinger ("Untersuchungen über die Wirkung d. Sandfilter d. Städtischen Wasserwerke in Zürich," 'Vierteljahrsch. d. Naturforsch. Gesellschaft in Zürich,' 34, 1889), C. Fraenkel and C. Piefke ("Versuche über die Leistungen d. Sandfiltration," 'Zeitsch. f. Hygiene,' 8, 1890, p. 1), C. Piefke ("Aphorismen über Wasserversorgung, Einrichtung und Betrieb von Filteranlagen," 'Zeitsch. f. Hygiene,' 8, 1890, p. 331), Proskauer ("Die Reinigung v. Schmutzwässern nach dem System Schwarzkopf," 'Zeitschr. f. Hygiene,' 10, 1891, p. 51). See our Appendix A.

surface soil—we know far too little about them to say much as to the comparison—the water is falling more and more out of the dangerous stages.

At still deeper levels, even these anaërobic forms are left behind, and the thoroughly filtered liquid may now subside into a closed subterranean basin, where it may remain pure for any length of time,* so far as living organisms are concerned, provided no fissures or direct prolongations of surface waters allow of contamination from above.†

It is obvious from the foregoing that the two great sources of contamination of our water-supplies are the air and the surface waters.‡

* The remarkably impure deep well water examined by Rohn and Wichmann ('Mitth. d. Oesterr. Versuchstat. f. Brauerei u. Malzerei,' H. 2) must surely have been connected with surface waters!

† A recent examination (1891) made by one of us of the water from deep wells in the chalk of the Kent Waterworks Company showed the number of micro-organisms revealed by the gelatine test to vary from 4 to 76 and to average 32 in 1 cubic centimeter. In all cases the water was taken directly from the pumps and before it had undergone any storage.

For collected results of the bacteriological examination of spring and well waters, see especially Hueppe ("Die hygienische Beurtheilung d. Trinkwassers vom biologischen Standpunkte," Schilling's 'Journ. f. Gasbeleuchtung und Wasserversorgung,' 1887), also Tiemann-Gärtner ('Untersuchung. d. Wassers,' Braunschweig, 1889, p. 498).

‡ It is hardly necessary now to insist on the importance of the air, and its dust, in this connexion. Reference may be made to the following in confirmation:—Angus Smith ('Air and Rain,' 1872), Tyndall ('Floating Matter of the Air,' 1881); Percy F. Frankland ("The Distribution of Micro-organisms in Air," 'Roy. Soc. Proc.,' 1886, No. 245, p. 509; "Some of the Conditions affecting the Distribution of Micro-organisms in the Atmosphere," 'Soc. of Arts Journ.,' 1887, vol. 35, p. 485; "A new Method for the Quantitative Estimation of Micro-organisms in the Atmosphere," 'Phil. Trans.,' 1887, B, p. 113); Grace C. and Percy F. Frankland ("Studies on some new Micro-organisms obtained from Air," 'Phil. Trans.,' 1887, B, p. 257); Percy F. Frankland and T. G. Hart ("Further Experiments on the Distribution of Micro-organisms in Air," 'Roy. Soc. Proc.,' vol. 42, 1886, p. 267); Miquel ('Annuaire de l'Observatoire de Montsouris,' 1877 to 1891, and 'Manuel Pratique d'Analyse Bactériologique des Eaux,' 1891). Also Aitken in 'Proc. Roy. Soc. Edinb.,' vol. 16, 1886, p. 139; 'Trans. Roy. Soc. Edinb.,' vol. 15, February 6, 1888; and as regards contamination by surface waters see Koch ('Rede zur Stiftungsfeier der militärärztlichen Bildungsanstalten,' 1888, p. 25), Plaggo and Proskauer ('Zeitschr. f. Hyg.,' vol. 2, p. 479), Soyka ('Deutsche Vierteljahrschr. f. öffentl. Gesundheitspflege,' 1888, p. 638), Wolffhügel ('Arb. a.d. Kaiserl. Gesundh.-amte,' 1886, p. 546), and Fraenkel ('Zeitschr. f. Hyg.,' 1889, p. 23).

As regards filtration, Percy F. Frankland ("On the Removal of Micro-organisms from Water," 'Roy. Soc. Proc.,' 1885, "New Aspects of Filtration and other Methods of Water Purification: The Gelatine Process of Water Examination," 'Journ. Soc. Chem. Ind.,' 1885; "Water Purification: its Biological and Chemical Basis," 'Proc. Institut. of Civil Engineers,' vol. 85, 1885–86; "Filtration of Water for Town Supply," 'Trans. of the Sanitary Institute of Great Britain,' vol. 8, 1886;

It is also clear that pathogenic forms find their access to such waters by the same routes as saprophytic and harmless ones, a point of primary importance when we reflect on the danger of such being in the air and the drainage of inhabited areas. That the spores of *Bacillus anthracis* find their way from the bodies of animals to the surface waters of meadows, and thence into rivers, must be accepted as proved by the researches of Pasteur and Koch,* and our knowledge in this connexion suggests only too plainly what may occur in other cases, thus explaining the observed facts that the microbes of typhoid, cholera, septicæmia, &c., do occur in exposed waters; and connecting the presence of other pathogenic forms, known to be cast off in secretions, dejecta, &c., with the suspicions aroused from the washing of milk vessels, &c., with such waters.

It is necessary to bear in mind, however, that although the vista of possibilities here opened out is a real one, most of the bacteriological examinations of water support the conclusion that by far the majority of the Schizomycetes met with in natural waters are harmless, or at least are not capable of producing disease directly in those who drink the waters.

Such conclusions have led to speculations, in different directions, as to why the bacteriological examination of waters has, so far, seldom led to the detection of pathogenic forms, although such waters are exposed to contamination.

Firstly, it is possible that a Schizomycete should lose its virulence or be weakened, or even die, when transferred from a suitable medium into one so thin and innutritious as any ordinary potable water would be; secondly, quite apart from the scarcity of food materials, it requires some reflection to thoroughly grasp how great must be the changes in the circumstances which a given pathogenic form—say, the anthrax bacillus, for argument—meets with when it leaves the living

"Recent Bacteriological Research in connection with Water Supply," 'Journ. Soc. Chem. Ind.,' 1887; "The Applications of Bacteriology to Questions relating to Water Supply," 'Trans. Sanitary Institute of Great Britain,' vol. 9, 1887); H. A. Nielsen ("The Bacteria of Drinking Water, in particular as regards the Species in the Water Supply of Copenhagen," Copenhagen, 1890; see 'Ann. d. l'Inst. Pasteur,' 1890, p. 41), Bertschinger ('Vierteljahrschr. d. Naturf. Gesellsch.,' vol. 34, 1889, also 'Ann. de l'Inst. Pasteur,' vol. 3, 1889, p. 692), Duclaux ("Le Filtrage des Eaux," 'Ann. de l'Inst. Pasteur,' 1889, pp. 41-56; and "Sur les relations du Sol et de l'Eau qui le traverse," 'Ann. de l'Inst. Pasteur,' 1889, pp. 172–184); also our Appendix A.

As to bacteria in ice, snow, and hail, see Prudden ('New York Med. Record,' 1887), Bordoni-Uffreduzzi ('Centralbl. f. Bakt. u. Parasitenkunde,' vol. 2, 1887), Janowski (*ibid.*, vol. 4, p. 547), and Schmelck (*ibid.*, vol. 4, p. 545). Fraenkel ('Zeitschr. f. Hyg.' vol. 1, pp. 302—314), and Odo Bujwid ('Ann. de l'Inst. Pasteur,' vol. 1, 1887, p. 592).

* Pasteur, 'Bull. de l'Acad. de Médecine,' 1880; Koch, 'Mittheil. a. d. K. Gesundheitsamte,' 1881, p. 49.

body of a sheep and is carried into a stream. In considering this example, the observed facts as to the susceptibility of anthrax to low temperatures should be borne in mind. The great reduction in temperature would alone suffice to impress it with effects very different from those of its previous environment—the tissues of a warm-blooded animal—and matters would be made no simpler by the differences in exposure to the oxygen of the air, the light of the sun, and so forth.*

That such a view is not without foundation is sufficiently proved by recent researches on the action of heat, light, and oxygen on this very bacillus in question.

To take the case of temperature first. It is generally agreed that *Bacillus anthracis* cannot go on growing and dividing below about 15° C., nor above about 45° C., and that it thrives best at some temperature near 35° C.; it is also agreed that it is markedly susceptible to the presence of free oxygen in its normal development. Although undoubtedly favoured by presence of oxygen, the anthrax bacillus will grow in the presence of only a very small quantity of air, (Liborius, 'Zeitschr. f. Hyg.,' 1, p. 170). Under favourable circumstances, but only if oxygen is present and the temperature fairly high, the bacilli form spores in their interior. This complicates the matter under discussion, for these spores are sometimes capable of remaining uninjured for long periods under conditions which would inevitably kill the vegetative rodlets.

Now Roux† has lately shown that in a given culture containing these spores some individuals are more resistant than others, and that when germinating it is of importance to a given spore whether it is near the surface of a liquid or deeper down; that at high temperatures, in contact with free atmospheric oxygen, the virulence of a given culture can be attenuated,‡ though no such attenuation results when out of contact with air.

These are by no means all the facts that have to be regarded, however.

* Possibly by far the most important of the destructive influences of fresh water on such microbes is that of the change in the conditions of osmosis, which is also entirely substantiated by experiment, and is in harmony with what we know of the physiology of living tissues (see Marshall Ward, "On Some Relations between Host and Parasite," &c., the Croonian Lecture for 1890, 'Roy. Soc. Proc.,' vol. 47, pp. 393—443, and references to the works of Pfeffer and De Vries therein; also Fischer, "Die Plasmolyse der Bacterien," in 'Ber. üb. d. Verhandl. Sächs. Gesellsch. Wiss. zu Leipzig,' vol. 1, 1891, pp. 52—74, and Wladimiroff, "Osmotische Vers. an lebenden Bakterien," in 'Zeits. Physik. Chem.,' vol. 7, pp. 529—543).

† Roux, "De l'Action de la Chaleur et de l'Air sur les Spores de la Bactéridie du Charbon" ('Ann. de l'Inst. Pasteur,' vol. 1, 1887, pp. 392—399).

‡ We ought to deal with this subject very cautiously, for others have stated, and some confuted, this previously; but of course we are not concerned with all the details here.

A large number of investigators, by means of researches first started by Downes and Blunt in 1877,* in this country, and carried on ever since by others, have shown that the action of the sun's rays has to be taken into consideration when dealing with questions of the vitality or rate of growth, &c., of the spores and rodlets of this and other Schizomycetes.

The controversy is too long for full treatment in this report, but the upshot of the whole may be summed up as follows. Certain rays of light, apparently more especially those known as the "chemical rays,"† so affect the germinating spores of certain bacteria (*Bacillus typhosus*, *Bacteria anthracis*), in presence of air, that their growth is inhibited. The presumption is that the solar rays enhance certain oxidation processes in the living protoplasm, but questions also arise in some cases as to possible effects on the nutritive media as well, though Janowski certainly seems to have eliminated these in his cultures of the typhoid bacillus.‡

A second possible view as to the fate of a given species of bacterium when suddenly washed into a stream is that it remains there unaltered, and that the chances are so enormously against its being detected, or (what, from some points of view, is the same thing) against its finding a suitable nidus in a living animal, that it simply wanders passively in the waste of waters surrounding it for an indefinite period, or until it reaches the sea.

This view also must be faced as one not altogether unsupported by observations, but only on the understanding that the microbe is in the spore stage, or, at least, passes into that condition soon after reaching the water, for the weight of bacteriological experience is distinctly against the probability of a living Schizomycete, in the simple vegetative condition, remaining as such for any length of time, at any rate in such a dilute medium as potable water.

With spores the matter is different. Duclaux found old spores of certain forms which had been kept out of contact with air for several years to be still capable of germination when sown in suitable

* Downes and Blunt, 'Roy. Soc. Proc.,' 1877, p. 488, and *ibid.*, 1878, p. 199.

† It should be clearly indicated, however, that the evidence goes rather to show that it is *insolation* which produces these results, and not diffused light. Insolation can have practically no effect in natural waters.

‡ For details as to the action of light on bacteria, consult Raum ("Der Gegenwärtige Stand unserer Kenntnisse ü. d. Einfluss des Lichtes auf Bacterien, &c.," 'Zeitschr. f. Hyg.,' vol. 6, 1889, pp. 312—368), for full references to literature to date. Then see Pansini ("Action de la Lumière Solaire sur les Microorganismes," in 'Rivista d'Igiene,' 1889; also 'Ann. de l'Inst. Pasteur,' vol. 3, 1889, p. 686); Janowski, ("Zur Biologie der Typhus-bacillen," in 'Centralbl. f. Bakt. u. Parositenk.,' 1890, Nos. 6—8); F. Elfving, 'Studien über die Einwirkung des Lichtes auf die Pilze,' Helsingfors, 1890, 139 pp. and 5 plates—deals more especially with fungi proper—and our Appendix A.

media,* and it is well known what extremes of temperature, &c., spores *can* withstand. At the same time, since the rule is that a spore germinates in even dilute solutions, when transferred thither, in presence of oxygen and if the temperature rises, it may be regarded as probable that, for aërobic bacteria at any rate, the changing conditions in a river, &c., will prevent its remaining merely passive—all available evidence is rather in favour of its either growing or else dying if it cannot adapt itself to the circumstances, although the death of spores may be delayed for many months and possibly even longer.

Indeed, recently, strong evidence has been produced, showing that pathogenic microbes may sink to the bottom of lakes and rivers and there remain in a living state, amongst the sediment or mud, for very long periods of time, until in fact, some flood or other disturbance causes them to become once more suspended in the water, when they may be carried by a stream or current to another place. It is obvious that this hitherto but little recognised factor is of the very highest importance in connexion with the supply of water from rivers subject to objectionable pollution.†

A third view is possible, viz., that the Schizomycete finds the new environment at least not unsuited to its immediate requirements, and that it grows and multiplies more or less successfully in the large mass of water.

This unquestionably happens with some forms, which, as we have seen, are so well adapted for life in rivers, ponds, and even pipes, that they have long been known as aquatic species.‡ As has been stated, and will be seen more clearly shortly, however, this is also true, to a limited extent, of many forms, including certain pathogenic species, which are only met with in natural waters as intruders; they are able to maintain themselves alive for variable periods, and then usually succumb.

Before passing to this part of the subject, we wish to remark upon the method for a long time employed in the bacteriological examination of water, and on some of the general results obtained.

Since 1881 it has been almost universally the custom to employ the gelatine-plate cultures as devised by Koch. A measured small quantity of the water to be examined is added to the nutrient gela-

* Quoted by Roux ('Ann. de l'Inst. Pasteur,' vol. 1, 1887, p. 392).

† Lortet and Despeignes, "Recherches sur les Microbes Pathogènes des Eaux Potables distribuées à la Ville de Lyon" ('Rev. d'Hygiène,' 12, 1890, No. 5); also Lortet, "Die pathogenen Bakterien d. tiefen Schlammes im Genfer See" ('Centralbl. f. Bakter.,' 9, 1891, p. 709).

‡ This term is, of course, not quite accurate, in view of the fact that *all* Schizomycetes must have water to grow; and are, indeed, descended from aquatic forms—lower Algæ.

tine, kept fluid at about 35° C., and the mixture, after solidification in a thin layer, is incubated generally at a temperature of 20—22° C. in contact with air, but protected from danger of contamination: we need not go into the particular methods of sterilisation, protection, incubation, &c.; suffice it to say that in a few hours or days colonies of bacteria appear on the culture plates, and the number of individual bacteria in the measured sample of water is estimated from these, on the assumption that each colony has sprung from one germ.

The comparison of numerous researches made in recent years, and the experience gradually being gained in all branches of the *technique* of the subject, have slowly led to the detection of numerous fallacies in the almost established mode of procedure.

In the first place, it was soon apparent that the mere *numbers* of bacteria, per cubic centimetre of water, are in no sense a satisfactory guide to the fitness of such water for domestic purposes; it may be quite true that one revolts from a water proved to yield large numbers of colonies of bacteria, and one can understand that a water yielding even 500 colonies per cubic centimetre should be preferred to one yielding, say, 5000 colonies per cubic centimetre, but, so long as this choice is based on the assumption that mere numbers decide the safety or danger of the water, it is utterly fallacious. The 500 colonies may contain some which have been developed from pathogenic germs, while the 5000 may have all arisen from harmless forms. This consideration entirely invalidates all the older conclusions, which were made in some quarters, as to a given water being good or bad according as it yields few or many colonies per cubic centimetre on plate cultures; the only test is to determine *what* the bacteria of the different colonies are, and the only general deduction of any value to be drawn from mere quantitative bacteriological determinations is, that a water obviously containing a number of different species is, on the whole, more likely to have been subjected to contamination than one which contains but few different kinds.

A water should be suspected, therefore, and subjected to further examination, if it yields several different kinds of colonies unknown to the investigator.

As a matter of practical experience, it is certainly impossible to rapidly identify more than a few colonies in such cultivations; if a complete investigation of the life histories, &c., of all the forms were attempted, the bacteriological examination of a single sample of water might take years, and consequently this part of the subject is the one which awaits and invites the attention of numerous and energetic, properly equipped workers.

Then as to the primary assumption which lies at the base of all the older plate cultures. This was that each colony has taken origin

from *one* germ, isolated at the time of infecting the gelatine, and which developed in the medium during the period of culture.

In the first place, the conclusion that each colony has sprung from one germ is a mere assumption, and it is to be viewed with suspicion at the outset. Cramer* showed that bacteria have a habit of sticking together in the water, and several other observers† have shown that this is a real danger in all bacteriological analyses, and that individual colonies often result from the growth, &c., of not one, but many agglomerated spores or segments. Many observers have attempted to get over this difficulty by shaking the sowing in distilled water, before infection. Wolffhügel and Riedel suggest the possibity that there are dangers connected with this method also, *e.g.*, removal of gases, oxidation, &c. It is even asserted that prolonged‡ mechanical shaking affects the growth of bacteria, but this concerns the transit, &c., of cultures rather than the point under discussion.

It has been suggested that the best method for ensuring separation from one another of the bacteria would be to pass the water through sterilised glass wool, as Elfving did for spores of fungi;§ only there would be some loss. Thoroughly sterilised cotton wool, or even filter papers, may be used, but there is danger of washing traces of soluble substances from these.

The question as to whether the colonies result from a single germ or from an agglomeration is, after all, not a matter of such grave importance as might at first sight appear, for if the precaution be taken, as it invariably should be, of violently agitating the sample of water immediately before making a plate cultivation, it is obvious that any conglomerate which may be present and does not yield to this treatment is, for practical purposes, a single source of infection, and will thus give rise to a single colony.

Another difficulty with plate cultures is due to some species causing liquefaction of the gelatine through the action of peptonising

* 'Kommissions-bericht über die Wasserversorgung von Zürich und ihren Zusammenhang mit der Typhus-epidemie des Jahres 1884,' Zürich, 1885, p. 92.

† Malapert-Neufville ('Zeitschr. f. analyt. Chem.,' vol. 25, 1886, p. 39), Wollfhügel and Riedel ("Die Vermehrung der Bacterien im Wasser," 'Arb. a. d. K. Gesundheitsamte,' vol. 1, 1886, pp. 455—480); also Fol and Dunant ('Revue d'Hygiène,' 1885, vol. 7, p. 183).

‡ It may be assumed that the shaking carried on for a few minutes only before making a culture can have no effect on the life of the microbes; even the effect of long-continued shaking is very doubtful, the evidence being quite conflicting. On this point see Horvath ('Pflüger's Archiv f. Physiol.,' vol. 17, 1878, p. 125), Naegeli ('Theorie d. Gährung,' 1879, p. 88), Reinke ('Pflüger's Arch. f. Physiol.,' vol. 23, 1880, p. 434), Büchner ('Sitzungsber. d. K. Bayer. Akad. d. Wiss.,' 1880, pp. 382 and 406), Wernich ('Desinfectionslehre,' 1880, p. 74), and further literature in these.

§ Elfving used cotton-wool ('Studien ü. d. Einwirkung des Lichtes,' p. 31). See also Geppert ('Ann. de l'Inst. Pasteur,' vol. 3, p. 673), who used glass.

ferments which they produce: this causes local floodings, and the running together of neighbouring colonies, or the submergence of some of them, and seriously interferes with the counting and estimation of the numbers.

The only mode of combating this difficulty consists in using such a volume of the infecting water as will yield a manageable number of such centres of liquefaction.

But these are by no means the only sources of fallacy incidental to the methods of gelatine-plate cultures. It has been implied by some of the earlier workers, rather than definitely assumed, that all the living germs in the sample of water mixed with the nutrient gelatine* will give rise to colonies, provided the plate culture is thin enough to ensure the access of oxygen to all parts, the sample small enough to ensure isolation of the individual bacteria or spores, and the temperature a suitably high one to promote rapid growth, without preventing the proper solidification of the medium.

As matter of fact, there are serious fallacies traceable to all these implications. Many bacteria are now known which are incapable of growing in presence of the oxygen of the air, while others will only withstand partial pressures of that gas; it may be safely concluded that the gelatine-plate cultures give no account whatever of these forms, although they may and often do occur in tap waters,† &c.

Moreover, even the thinnest layer of gelatine may so far hinder the access of oxygen to completely submerged aërobic forms as to retard their growth, and so they become dominated by the more rapid development of other colonies. This domination is not necessarily due to the mere flooding of the suppressed forms with liquefied gelatine: Garré‡ showed a short time ago that some bacteria, growing on gelatine side by side with other species, can inhibit the life-actions of the latter by the poisonous influence of their metabolic products, and Miquel§ claims to have proved similar actions in water, and even to have isolated the toxic principles, and rendered other water immune by their aid.

* The quality of gelatine and peptone varies also. For hints in this connexion see Reinsch, "Zur bakteriolog. Unters. des Trinkwassers" ('Centr. f. Bakt.,' vol. 10, 1891, p. 415).

† A good instance has recently been investigated by Perdrix ("Sur les fermentations produites par un Microbe anaérobie de l'Eau," 'Ann. Inst. Pasteur,' vol. 5, 1891, pp. 286—311).

‡ "Ueber Antagonisten unter den Bakterien" ('Correspondenzbl. f. Schweizer. Aerzte,' Jahrg. 17, 1887). Also Blagovestchensky ("Sur l'Antagonisme entre les Bacilles du Charbon et ceux du Pus Bleu," 'Ann. de l'Inst. Pasteur,' 1890, vol, 4, pp. 689—715).

§ "Dixième Mémoire sur les Poussières organisées de l'Air et des Eaux" ('Annuaire de l'Observatoire de Montsouris,' 1888), and 'Manuel Pratique d'Analyse Bactériologique des Eaux,' 1891, pp. 153—155.

Again, as is well known, there are several forms which will not grow on gelatine at all,* and there are others which grow so slowly that they will not be counted in the estimations made by gelatine-plate cultures, either because the colonies formed in the time are too small to be seen, or because they succumb to dominant forms—for it must never be forgotten that, among competing Schizomycetes, it is especially the early forms which gain the advantage, as elsewhere in nature.

Finally, since the temperature has been shown to be such a determining factor in the growth and multiplication of bacteria, we may be sure that this item affects these plate cultures also, and it is well known that different numbers are obtained according to the temperature of incubation, and with reference to this point it is especially to be noted that the optimum temperatures for different bacteria may differ considerably.†

Taking all the facts into consideration, therefore, it is necessary to regard the gelatine-plate method as an imperfect one at best. But if we inquire whether there is a better one, we are bound to reply that there is not, at any rate for general purposes; but for special requirements it is possible to devise several modified methods for the culture of particular forms, and this has been done in certain cases.

§ III. *The Vitality of Micro-Organisms in Water.*

It is now time to enter upon the special literature dealing with the behaviour of selected forms of Schizomycetes in particular samples of water, and we propose to treat this somewhat more in detail, and in chronological order, so far as possible, because it bears directly on the subject of our enquiry.‡

This invesigation followed as the natural corollary to the discovery that some micro-organisms can multiply to a most extraordinary extent in waters almost entirely destitute of organic matter, like distilled water. The first recorded instance of such multiplication

* Some of these are of the highest importance in connexion with the chemical changes taking place in natural waters, *e.g.*, the nitrifying organisms (Percy F. and Grace C. Frankland, "The Nitrifying Process and its Specific Ferment," 'Phil. Trans.,' 1890, B, p. 107; Winogradsky, 'Ann. de l'Inst. Pasteur,' 1890 and 1891; Warington, 'Chem. Soc. Journ.,' 1891, p. 484).

† In this connexion it should be noted that the range of temperature for different bacteria is much larger than is commonly assumed. There are species which will grow at 0° C., and there are others which grow at temperatures as high as 50—70° C. See Fischer ("Bakterienwachsthum bei 0° C., &c.," 'Centr. f. Bakt.,' vol. 4, 1888, p. 89), Globig ('Zeitschr. f. Hyg.,' vol. 3, p. 294), Forster ('Centr. f. Bakt.,' vol. 2, p. 337), Miquel ("Monogr. d'un Bacille vivant au-delà de 70° C.," 'Ann. de Micrographie,' Année I, Paris, 1888, pp. 4—10).

‡ See Appendix C, p. 268.

in distilled water was made by one of us in 1885,* on which occasion it was found that in the course of forty-eight hours the number of microbes had increased from 1073 in the cubic centimetre to 48,100 in the same volume. Similar instances of multiplication in the pure spring water supplied to Munich were published shortly afterwards, by Leone,† whilst the same phenomenon was observed by Cramer‡ in the case of the microbes present in the waters of the Lake of Zürich.

Similar phenomena formed the subject of more extensive investigations contained in three memoirs, which appeared almost simultaneously in 1886 by Wolffhügel and Riedel,§ by Meade Bolton,‖ and by one of us.¶ Each of these not only confirmed the rapid and extensive multiplication of microbes, even in the purest natural waters, but differed from the predecessors in recording the results of experiments in which specific pathogenic forms were introduced into natural waters of different kinds, including sewage. It will be convenient to discuss, in the first instance, these three contributions to the subject along with another by Heraeus, which appeared shortly afterwards.

In Meade Bolton's paper,** after referring to the literature regarding the general bacterial contents of ordinary waters, and criticising the various methods hitherto in vogue, the following generalisations are made :—

1. Ordinary waters always contain some bacteria.

2. The numbers of individuals and species vary in different waters, and from time to time.

3. Certain forms predominate, because they can multiply readily in such waters, as is proved by their rapid increase when the water is allowed to stand for a few days.

4. After the climax of increased numbers has been obtained, the bacteria gradually diminish in quantity.

Bolton established the truth of these conclusions, and showed that the growth and increase of these water bacteria differ according to

* Percy F. Frankland, "The Removal of Micro-organisms from Water," 'Roy. Soc. Proc.,' vol. 38, 1885, p. 387.

† "Sui Micro-organismi delle Acque Potabili, loro Vita nelle Acque Carboniche," 'Rendiconti della R. Accademia dei Lincei,' 4 Ottobre, 1885; 'Chem. News,' vol. 52, p. 275.

‡ 'Die Wasserversorgung von Zürich, ihr Zusammenhang mit der Typhusepidemie d. Jahres 1884,' Zurich, 1885.

§ 'Arbeiten a. d. K. Gesundheitsamte,' vol. 1, pp. 455—480.

‖ 'Zeitschr. f. Hyg.,' vol. 1, p. 76.

¶ Percy F. Frankland, "The Multiplication of Micro-organisms," 'Roy. Soc. Proc.,' vol. 40, 1886, p. 526.

** "Ueber das Verhalten verschiedener Bakterienarten im Trinkwasser" ('Zeitschr. f. Hyg.,' vol. 1, 1886, pp. 76—114).

the kind of water, the temperature and other external conditions remaining the same.

He gives examples showing that the increase is most rapid, as a rule, during the first thirty-six hours, and then a diminution sets in, day after day, ending in the water containing a smaller number than at first.

This gradual diminution was not due to mere precipitation, but was, perhaps, in part to be accounted for by the coherence of the germs in clumps, and in part to actual death.

He then isolated and described sixteen of the commonest species, which were shown to actually grow and multiply in ordinary drinking waters.

Two of these forms were shown to be capable of easy multiplication in such waters, and that quite independently of the chemical constitution of the waters. He found that they flourished in the purest distilled water he could obtain, as well as in "bad" water, and assumes that this is because the very small amount of organic nutriment they demand* is never absent.

Bolton concluded from his experiments that variations of temperature, and in the amount of oxygen dissolved in the water, were far more important factors than the chemicals dissolved in ordinary waters. In this conclusion, however, he is neither supported by his own nor the previous experiments of Leone, for it was found by both that the multiplication was almost equally rapid if a stream of hydrogen or a stream of air was passed through the water.

He further inferred that, in practice, the accumulation of bacteria in pipe waters is due to the multiplication of forms (carried in by surface drainage in the first instance) in the standing water as the temperature rises.

He then tried the effects of (1) distilled water, (2) common drinking waters, and (3) badly contaminated waters, on specific pathogenic and other bacteria, obtained from pure cultures and added to the waters with as little traces of the culture medium as possible. In many of his experiments, however, he has apparently failed to secure this last-named condition, as in most cases the number of organisms introduced into the particular waters is recorded as "un-countable," thus clearly pointing to insufficient dilution before inoculation. To save repetition it must be mentioned that all of Bolton's experiments were made with waters previously sterilised by heat.

* It is, however, much to be regretted that in not a single instance is the chemical composition of any of these waters recorded by the author.

It may be observed here that we cannot accept, without reserve, general statements to the effect that *pure* water is capable of supporting the life of bacteria. Miquel shows how such water, which may be obtained in quantity by simple condensation ('Analyse Bact. des Eaux,' p. 156), is incapable of supporting bacterial life (pp. 157—158).

He found that spore-free bacilli of *Bacillus anthracis* rapidly die off in tap water; whereas, in the condition of spores, this organism may remain alive in such waters for nearly a year.

Staphylococcus pyogenes aureus may live for from ten to twenty, to upwards of thirty days in ordinary and bad waters respectively.

Micrococcus tetragenus rapidly disappeared in every case in less than four to six days.

Bacillus typhi abdominalis lived in some cases upwards of fourteen days in the absence of spores, and upwards of thirty to forty days in the form of spores.

In all these cases the two most important factors were the presence of spores and the temperature; the bacteria were eliminated the more rapidly the higher the temperature; they resisted the longer the more they had matured their spores. This was true, assuming that no proteids or other assimilable organic bodies were added to the water.

None of Meade Bolton's experiments were made with simultaneous presence of water bacteria: on the whole, subsequent researches have shown that the presence of forms which easily flourish in ordinary waters hurries the elimination of the intruding pathogenic forms.

Heraeus,* in his general conclusions supports those of Meade Bolton in most of the important respects, and especially the rapid increase of individuals of the bacteria in ordinary waters. He insists on the fact that certain forms multiplied in solutions absolutely devoid of organic materials.

At the same time, he concludes from his experiments that a "bad" water suits the bacteria better than good drinking water; and this conclusion is of course in accordance with all ordinary experience, since a "bad" water contains relatively much organic matter.

The important paper by Wolffhügel and Riedel,† which may next be examined, is full of excellent hints on methods, and of references to work bearing on the subject, and contains several valuable warnings as to sources of error in such investigations. The authors insist that the composition of the water is of importance, and in this respect place themselves in direct antagonism to Meade Bolton.‡ They employed both pathogenic and non-pathogenic bacteria, and sterile as well as normal waters.

* "Ueber das Verhalten der Bacterien im Brunnenwasser, sowie über reducirende und oxydirende Eigenschaften der Bacterien" ('Zeitschr. f. Hyg.,' vol. 1, 1886, pp. 193—234).

† "Die Vermehrung der Bacterien im Wasser" ('Arb. a. d. Kaiserl. Gesundheitsamte,' Berlin, 1886, vol. 1, pp. 455—480).

‡ We may here emphasise our previous note that most investigators disagree with Meade Bolton in this respect, and we may conclude that the latter lays too little stress on the constitution of the water.

These authors prove that very minute traces of organic materials in a water induce rapid multiplication of certain species, that the temperature and quiescence of the water are important, and discuss the question as to the effects of mechanical shakings* of the waters, *e.g.*, in transit. They leave this matter undecided, but are of opinion that it will have to be reckoned with by bacteriologists.

As regards pathogenic species, they found that *Bacillus anthracis* multiplied in the dirty water of the River Panke, both normal and when sterilised by heat,† so long as the temperature was high enough (12—15° C. to 30—35° C.); but that at low temperatures the bacilli did not flourish, and even died off.

Typhoid bacilli ("ileo-typhus") lived for some time, and even multiplied, in (sterilised) ordinary drinking water, as well as in the (sterilised) bad waters. In distilled water they gradually died out, though they may require twenty days or more to do so. In non-sterilised waters they found that they grew so slowly that they were swamped by other forms on the gelatine-plate test-cultures, thus showing how difficult it is to obtain a satisfactory result in such experiments.

To obviate this difficulty they placed the typhoid bacillus in selected waters, containing *selected* water bacteria, and they then found that it lived so long that they felt constrained to warn us that this dangerous form may maintain itself for weeks: they also prove that milk‡ is a good vehicle for it, and that the belief in the danger of washing milk-cans with water containing typhoid bacilli is a well founded one.

Cholera spirilla were found to maintain themselves for seven days at least in all kinds of sterile waters, and to be still present in some cases even after eighty-two days. In unsterilised waters, however, this form is soon overcome. Here, as in other cases, the temperature was found to be important.

A curious result is worth noting. They found that the cholera spirilla take some time to accommodate themselves to the exigencies of a water containing competing forms, and consequently the latter usually dominate and eliminate the former. But in certain cases the cholera spirilla do accommodate themselves to the circumstances for a time, and if such specimens be then removed and placed in a fresh sample of the water they multiply at once, and are much more

* With full reference to previous literature.

† None of the experiments were made with water sterilised by *filtration* only.

‡ See also W. Hesse ("Unsere Nahrungsmittel als Nährböden für Typhus und Cholera," 'Zeitsch. f. Hyg.,' vol. 5, 1889, p. 527); Kitasato ("Das Verhalten der Cholerabacterien in der Milch," 'Zeitsch. f. Hyg.,' vol. 5, 1889, p. 491); Almquist ("Neue Erfahrungen über Nervenfieber und Milchwirthschaft," 'Zeitschr. f. Hyg.,' vol. 8, 1890, p. 137).

resistant to the water bacteria among which they find themselves.

In distilled water the cholera spirilla usually died off rapidly,* but cases happened where they lived for thirty-three days; possibly the distilled water contained impurities in these cases.

The authors insist on the danger of cholera germs in water, and expecially in ordinary river, well, and tap waters, which showed the presence of living cholera germs *seven months after infection.*

In the paper by one of us† the particular waters submitted to examination were those of the rivers Thames and Lea before and after filtration by the several London Companies, as well as the deep-well water from the chalk supplied by the Kent Company. It was found that the microbes in the unfiltered waters underwent but little multiplication, and in some cases very considerable diminution, on standing at the ordinary temperature of the air, whilst at a temperature of 35° C. very rapid multiplication took place, which was followed by subsequent decline. In the case of the filtered river waters, on the other hand, there was invariably a large increase, especially at the high temperature, also followed, however, by a subsequent decline. By far the most rapid multiplication was observed in the case of the organically pure deep-well water; thus on one occasion the numbers rose from 7 to 495,000 in the course of three days when the water was kept at 20° C.; the tendency to a subsequent decline was, however, also exhibited. On these surprising results the author points out that the deep-well water is at the outset almost wholly free from micro-organisms, and that it has never before been inhabited by such living matters, and that it is only reasonable to infer, therefore, that those of its ingredients which are capable of nourishing the particular micro-organisms which flourish in it are wholly untouched, whilst in the case of the river waters, the most available food supply must have been largely explored by the numerous generations of micro-organisms which have inhabited them. Further, he remarks that the number of different varieties of micro-organisms is far greater in the case of the river waters than in that of the deep-well water, and that in the latter case, therefore, the organisms present will probably have a freer field for multiplication than in the presence of competitors, some of which may not improbably give rise to products which are hostile to others.

In a similar manner he explains the greater capacity for multiplication exhibited by the filtered as compared with the unfiltered river

* As the authors point out, this agrees with Babes' results (Virchow's 'Arch. f. Path. Anat.,' vol. 99, 1885, p. 152), and contradicts those of Nicati and Rietsch ('Revue d'Hyg.,' 1885, No. 5, p. 353). Since the latter employed liquid cultures of the bacilli, they probably introduced food materials into the water.

† *Loc. cit.*, p. 471.

water, for by the process of filtration the number of different varieties of micro-organisms is largely reduced, as is at once seen by the inspection of the plate cultivations, and those varieties which remain have, therefore, a more favourable opportunity for reproduction than in the presence of more numerous varieties.*

The specific forms experimented with were the *Bacillus pyocyaneus* (from green pus), Finkler-Prior's *Spirillum*, and Koch's *Spirillum of Asiatic cholera*; they were in all cases introduced into sterilised waters only.

The *Bacillus pyocyaneus* was found to multiply extensively in distilled water, filtered Thames water, deep-well water, and London sewage. Finkler-Prior's *Spirillum*, on the other hand, exhibited a most extraordinary susceptibility to immersion in water, for in none of these waters could its presence be demonstrated after the first day.

The results obtained with Koch's *Spirillum of Asiatic cholera* were particularly instructive, for when this was taken from a weak culture in gelatine, the spirilla were no longer demonstrable after the first day in the infected waters, but when the spirilla of the same cultivation were revivified by cultivation in broth and then introduced into the aqueous media they were found to multiply abundantly in the sewage, whilst in the deep-well and filtered Thames water they underwent numerical reduction in the first instance, followed by slight multiplication, which was again succeeded by decline, and on the ninth day they were still demonstrable. A temperature of 35° C. caused their more rapid destruction, as confirmed by the results of other investigators.

In a later paper by one of us† the author finds that the *cholera spirilla* have remained alive for eleven months in the sterile sewage, and in further experiments with *Bacillus anthracis*, he found that in sterile distilled and in sterile filtered Thames water the organisms remained alive for upwards of sixty days, a considerable diminution taking place during the first days, after which the numbers remained practically constant. The initial diminution, he suggests, is due to the dying off of the bacilli, the spores alone surviving. In sterile London sewage *Bacillus anthracis* underwent considerable multiplication. Experiments were also made with the *Streptococcus of erysipelas* (Fehleisen), which was apparently destroyed within one hour in distilled water, but lived from two to five days in sterile filtered Thames water, and two days in sterile London sewage.

* These results are partly in accordance with, and partly contradictory of, Miquel's observation that the action of numerous Schizomycetes in a water may render that water "immune" to infection by other Schizomycetes, as quoted in the footnote to p. 203.

† Percy F. Frankland, "Recent Bacteriological Research in connection with Water Supply," 'Journ. Soc. Chem. Industry,' 1887.

Kraus contributed a valuable paper in 1887.* After pointing out that, important as are the results of Meade Bolton, and of Wolffhügel and Riedel, to science, they have very little practical utility, because (1) they concern chiefly sterilised waters, which do not occur in the open, and (2) the temperatures employed were too high to be compared with what happens in daily life, this author proceeds to describe his results with ordinary drinking waters kept at about 10·5° C.

He found that the typhoid bacillus under these circumstances soon succumbs to the rapidly increasing "water forms," and that it was eliminated in seven days.

Koch's cholera spirillum could not hold its ground more than two days at this temperature, in contest with the rapidly dominating aquatic forms.†

Even *Bacillus anthracis* disappeared from these waters in four days.

Kraus concludes that much more must be put down to the direct effect of the competing bacteria in such cases, than to the quality of the water or the original number of forms contained in it.

We may remark in this connexion that it bears out what is also deducible from the preceding results of Bolton and Wolffhügel and Riedel,‡ and further, that this view of Kraus is distinctly supported by our knowledge of the competing action of dominant forms, due to their successful seizure of oxygen, food materials, &c., on the one hand, and to the toxic actions of their metabolites on the other.§ It is not improbable that sterilisation by heat acts both by setting free food materials in the form of dead bacteria, as well as by destroying such toxic principles.

Gärtner‖ found that typhus bacilli will *live* for long periods in

* "Ueber das Verhalten pathogener Bacterien im Trinkwasser" ('Arch. f. Hyg.,' vol. 7, 1887, p. 234).

† These and similar results with mixtures of microbes must be received with great caution, as already pointed out, for it has been proved by Gruber ('Wiener Mediz. Wochensch.,' 1887, Nos. 7 and 8) that on placing the cholera spirilla in contact with ordinary putrefaction bacteria, the latter, in the first instance, gain an enormous numerical ascendency over the cholera spirilla; but if the struggle between the two be sufficiently protracted, the cholera spirilla can, at the close of the putrefaction process, be still found in the living state.

‡ Bolton, however, made no experiments with unsterile water; and Wolffhügel is far from convinced as to the *destruction* of typhoid bacilli by water bacteria, putting down their absence on the plate cultures rather to experimental difficulties of finding them. It is not impossible that the experimental difficulties account for these results of Kraus.

§ See also note on p. 203 regarding Garré and Miquel's results.

‖ "Pathogene und Saprophytische Bakterien in ihrem Verhältniss zum Wasser, insbesondere zum Trinkwasser" ('Correspondenz-Blätter des allgem. Aerztl. Vereins von Thüringen,' 1888, Nos. 2 and 3).

waters, but concluded that it does *not multiply* in them unless appreciable quantities of organic food materials are present: he found that $\frac{1}{100}$th part of bouillon added to the water induced vigorous growth and multiplication.

He concludes that cholera germs cannot multiply in ordinary waters, under ordinary conditions; but the temperature, the nature of the competing bacteria, and the vitality of the cholera germs themselves affect the question.

Among other factors which influence the life of microbes in water, carbon dioxide may be assumed to be of importance: most forms are influenced more or less adversely, whilst perhaps some are not susceptible to its presence.* Light is of no consequence in this respect.

Ferrari, in a paper dealing with the effect of various fluids employed in surgery on pathogenic organisms, observed that *Staphylococcus pyogenes aureus* rapidly multiplies in distilled water,† and this to such an extent that the effects were observable during several days, and the numbers were so large by the fifth day that they could no longer be estimated.

At the same time, the preliminary diminution of numbers during the first hours or days in these cases (and in similar experiments of numerous other observers already referred to) suggests the suspicion that some of the increase at least must be attributed to the

* Kolbe ('Journ. f. Praktische Chemie,' N.F., 1882, vol. 26, and 1886, vol. 28) had already pointed out the antiseptic action of carbonic anhydride, in connexion with the preservation of beef, and Leone (*loc. cit.*) showed how the number of microbes in water underwent rapid diminution on saturating the latter with carbonic anhydride at ordinary pressures; although the complete destruction of germs cannot be relied on by this agency, it points, at any rate, to the greater safety of carbonated waters, more especially if they have been kept for some time in stock. Systematic experiments have also been made on the action of carbonic anhydride on specific micro-organisms, pathogenic and harmless, by C. Fraenkel ("Einwirkung der Kohlensäure auf die Lebensthätigkeit der Mikro-organismen," 'Zeitschr. f. Hyg.,' vol. 5, 1889, p. 332), and by one of us (Percy F. Frankland, "On the Influence of Carbonic Anhydride and other Gases on the Development of Micro-organisms," 'Roy. Soc. Proc.,' vol. 45, 1888, p. 292, 'Zeitschr. f. Hyg.,' vol. 6, p. 13). These investigations show that by far the greater number of known bacteria, both pathogenic and otherwise, have their growth arrested by carbonic anhydride, although many of them subsequently revive on exposure to air. The most important papers on the effect of this gas, in mineral waters, on bacteria are Hochstetter ("Ueber Mikro-organismen im künstlichen Selterwasser nebst einigen vergl. Unters. ü. ihr Verhalten im Berl. Leitungswasser u. im dest. Wasser," 'Arb. a. d. Kais. Gesundheitsamte,' vol. 2, 1887, H. 1 and 2); Reinl ("Die gebräuchlichsten kohlensäurehaltigen Luxus- und Mineral-Wässer vom bakteriol. Standpunkte," 'Wiener Med. Wochenschr.,' 1888, Nos. 22 and 23); Fazio ('I Microbi delle Acque Minerali,' Naples, 1888).

† "Ueber das Verhalten von Pathogenen Mikroorganismen in den subcutan einzuspritzenden Flüssigkeiten" ('Centr. f. Bakt.,' vol. 4, 1888, p. 744). It should be pointed out that in this respect his results are in direct antagonism to Meade Bolton's with the same organism.

decomposition of those which die when first put into the water. Braem has shown pretty clearly that in some cases, at any rate,* distilled water kills anthrax and cholera bacilli, as well as *Staphylococcus pyogenes aureus*, though not always rapidly. Thus cholera lived for 24 hours, anthrax from 8 to 12 days, while the *Staphylococcus* required 25 to 50 days for its elimination.

Braem says that the typhoid bacilli were still active after 60 days in distilled water, and were not eliminated till 188 days had passed.

In the present state of our knowledge such results can most reasonably be explained on one of the three following assumptions:—(1) Either the distilled water was not pure (*i.e.*, it was contaminated in the still, or more probably by food materials carried in during infection); or (2) the products of decomposition of the dead and dying bacteria during the sojourn in the water, afforded food materials for the rest. Most probably both sources of error occur in those cases where the increase is very marked and prolonged: of course the products of decomposition of previously living bacteria would only account for a smaller number than the original, *i.e.*, the usual case. Whilst (3) the possibility may be suggested, that the progeny formed in the distilled water is of a degraded order, in which the individuals have a smaller dry body weight than the original forms introduced.

Uffelmann,† experimenting with the waters of Rostock, finds that typhoid bacilli, at ordinary temperatures, can hold their own for from several days to two weeks; and that *Bacillus anthracis* remained alive for three months. Although cholera germs are much less resistant, yet they, also, may be carried in such waters as are used for domestic purposes.

Karlinski‡ investigated the bacteria of five Innsbruck waters, and then determined their normal behaviour at 8° C.: in all, the numbers of Schizomycetes increased when the water was allowed to stand at this temperature. He then infected these waters with the bacilli of typhoid, cholera, and anthrax, and kept them also at 8° C., and found that these all diminished rapidly in numbers in the struggle with the increasing and eventually dominant water forms. Cholera could not maintain itself for three days, typhoid not beyond six days, and anthrax three days at the given temperature. It will be noticed that this is a valuable confirmation of Kraus's results at 10·5° C.

* "Recherches sur les Phénomènes de Dégénérescence des Bactéries Pathogènes dans l'Eau Destillée" (Ziegler's 'Beitr. zur Pathol. Anat.,' vol. 7, H. 1).

† "Trinkwasser und Infections-krankheiten" ('Wiener Medicinische Presse,' 1888, No. 37).

‡ "Ueber das Verhalten einiger pathogener Bakterien im Trinkwasser" ('Arch. f. Hyg.,' vol. 9, 1889, p. 113).

In a second paper,* Karlinski goes more deeply into the question of the maintenance of the typhoid bacillus in a natural water, working in the open in order to avoid the errors due to confined samples in the laboratory. The temperature, chemical constitution, and bacterial contents of the water were examined, and the well was then infected with a bouillon culture of typhoid germs.

Daily examination of the contents, continually stirred to prevent precipitation, showed a rapid increase of the normal water bacteria, and a corresponding decrease of the typhoid bacilli, till none of the latter remained after fourteen days.

The chemical constitution of the water, examined daily, also restored itself during the fourteen days through which the infection lasted. Other experiments confirmed these results.

§ IV. *Summary and Conclusions.*

If we now try to put together the results of the various investigations referred to, it is evident that the inquiry into the vitality of micro-organisms in ordinary waters is by no means to be carried out merely by putting such germs into a given water, leaving them there for a time, and simply determining their relative increase or decrease during a given period.

The first fact to be firmly grasped is that water, as met with in actual life, is a very variable medium indeed; and that even when it is admitted that such rough distinctions as are implied by the names river water, spring and well water, distilled water, soft and hard water, and so on, classify the subject but imperfectly, the matter is by no means ended. Not only are no two river waters alike in constitution, but probably no two samples of distilled water are absolutely so when the original water has been taken from different sources in the first instance.

The second great fact to be clearly apprehended is that a Schizomycete is not only a very minute organism, but that it requires correspondingly minute traces of food materials for its nutrition: consequently there is less cause for surprise than is sometimes expressed at the existence of such large numbers of these micro-organisms in a natural water which has passed over the soil in contact with the atmosphere, and attained an ordinary temperature.

A less obvious truth—but one that must be insisted upon—is that a Schizomycete is an extremely delicate organism, simply because it is a living being, and therefore its reactions to a medium such as an ordinary water are far more delicate and complex than those of the usual chemical reagents: furthermore, and this is one of the most

* "Ueber das Verhalten des Typhus-bacillus im Brunnenwasser" ('Arch. f. Hyg.,' vol. 9, 1889, p. 432).

important points of all, the living Schizomycete is a variable factor in itself, because it has a variable organisation.* When, therefore, we place bacteria in water, we must not expect the resulting reactions to be constant.

The matter is obviously rendered still more complex when we turn a given species of Schizomycete into a water already peopled with aquatic (and, therefore, presumably well adapted) forms of different species; for the whole teaching of biology shows that the competing organisms cannot exist side by side without affecting the welfare of each.

If we assume the simplest case, for the sake of argument, we have to remember at least the following:—

(1.) The water itself affects the living speck of protoplasm we place in it, not only mechanically, but more especially physically and chemically.†

(2.) The gases dissolved in the water exert pronounced effects, as is known from the relations of oxygen and carbon dioxide to plant life in general, and from the effects of these and various other gases on bacteria in particular.

(3.) Any dissolved or suspended substances in the water must exert definite actions on the living organism. This applies not only to the minerals and organic substances in solution which are directly useful as food materials, but also to products of metabolism or of other chemical changes which are injurious to the life of the protoplasm of the microbe. Moreover, it applies to suspended particles which exert surface attractions towards the suspended micro-organisms,‡ or which affect the water in any way.

(4.) The temperature of the water is, as has been seen, of the utmost importance for the life or otherwise of any given species; and it requires but a moment's consideration to see that this factor exerts an important influence on all the preceding.

(5.) Although we are still very ignorant of the relations of light to this subject, it is at any rate clear that in some cases at least certain rays of light may complicate matters when they fall in suffi-

* Proofs of this will suggest themselves to every biologist. We need simply refer to Roux's experiments with anthrax ('Ann. de l'Inst. Pasteur,' 1887, p. 392), and to those of Wolffhügel and Riedel ('Arb. a. d. Kais. Gesundheitsamte,' 1886, p. 455) with cholera.

† As regards this we may call attention to the plasmolysis experiments of De Vries, Pfeffer, Fischer, and Wladimiroff already referred to on p. 198.

‡ There is a large literature on this subject (and the allied one of filtration). See Percy F. Frankland ("The Removal of Micro-organisms from Water," 'Roy. Soc. Proc.,' 1885, pp. 379—393), and Krüger ("Physikalische Einw. v. Sinkstoffen auf die im Wasser befindl. M'organismen," 'Zeit. f. Hyg.,' vol. 7, 1889, pp. 86—114); also Duclaux ("Le Filtrage des Eaux," 'Ann. de l'Inst. Past.,' 1890, pp. 41—46), where other references are given.

cient quantity on water containing bacteria in suspension, and organic substances in solution, but it is not probable that this forms an important factor in the case of natural waters which cannot be subjected in their entirety to direct insolation.

(6.) The evidence is still less conclusive as regards mechanical disturbances in the water, so far as they directly affect the living cells of the micro-organisms, but it is at least highly probable that every wind-raised wave, every tumble over a fall or weir, and every pause in a backwater or lake, must affect the matter, if only in so far as it alters the gaseous contents of the water, or the relative distances between the individual micro-organisms.

Enough has been said to show that the bacteriologist who attacks the question before us must at least bear these facts in mind.*

Now, as to the questions of distilled as opposed to non-distilled water, and of sterilised as opposed to non-sterile waters.

It will be conceded forthwith that distilled, and we will assume pure, water offers little scope for practical enquiry. Such water is unknown in nature, except momentarily or in inaccessible forms, and the only lessons to be expected from its action on bacteria are of purely scientific and philosophical interest; distilled water, therefore, should be used in check experiments, and the results compared with those obtained with other waters, not forgetting that "distilled water" is not a constant medium.

As to experiments with sterilised water, the matter is very different, for most observers are unanimous as to the longer vitality of pathogenic forms in sterilised water than in the same water before sterilisation; the experiments in sterilised water may thus furnish us with the ultimate limits of vitality, and will, therefore, act as valuable guides.

It must, however, be remembered that there are two ways of sterilising a water,† (1) by heat and (2) by filtration. In both cases the constitution of the water may be altered. Where heat is employed the gases are driven off, in whole or in part; soluble products may be rendered insoluble, *e.g.*, carbonates precipitated; the proteids, &c., of the killed micro-organisms are placed more or less at the disposal of the living ones which follow; and the solution (which a natural water really is) becomes more concentrated.‡ Moreover, many meta-

* We have not thought it necessary to discuss the question as to the action of electricity on bacteria: the results hitherto are negative, excepting in so far as electrical currents alter the chemical constitution of the medium. For literature and criticism see Duclaux ("Action de l'Électricité sur les Microbes," 'Ann. de l'Inst. Past.,' vol. 4, 1890, pp. 677—680).

† It is obviously unnecessary to discuss sterilisation by means of antiseptic and poisonous substances, although this is, of course, of great importance in connexion with the treatment of sewage, &c.

‡ Though, of course, there is not *necessarily* diminution in volume in the process of sterilisation.

bolites of the nature of ptomaines and the like must be altered or destroyed.

Filtration, through porous films of porcelain, certainly acts less violently on the water; but it must by no means be concluded that either the chemical constitution or the physical character of such filtered water is absolutely unaltered. In the case of ordinary filtration one series of changes alone, viz., the alteration of the proportions of the gaseous contents owing to the difference of pressure on the two sides of the filtering film, will illustrate this.*

On the whole, however, we may conclude that in cases where it is necessary to eliminate the living bacteria of a natural water, the process of filtration through porous porcelain is a better method than that of sterilisation by heat.

There can be little doubt that some of the discrepancies between the results of the various observers, referred to on pp. 204—214, are chiefly due to the sources of difference here indicated.

It may be concluded, with some show of certainty: (1) That the numerical results obtained by the gelatine-plate method, are, on the average, too low.

(2.) That several workers employed temperatures too high for comparison with what occurs in natural waters in this country.

(3.) That many of the results are vitiated by small quantities of very concentrated food materials having been introduced into the waters with the pathogenic germs employed for infection.

(4.) That the conclusions drawn from experiments with distilled water must be received with great caution, and are of little practical value. On the whole we may regard "pure" water as a worse medium for the life of pathogenic bacteria,† in spite of apparently contradictory results in the hands of some of the investigators.

(5.) That the conclusions drawn from cultures in sterile waters must also be received with due regard to all the facts, and especially those where the water was sterilised by heat.

On the other hand, the enormously greater experimental difficulties attending the investigations in which unsterilised waters are used necessitate that the results should also be very carefully scrutinised, and should not be finally accepted until confirmed by numerous investigators attacking the question from different points of view, and using different methods of research.

(6.) That it is not safe to regard mineral waters as necessarily

* For the effect of filtration through porous porcelain on the chemical composition of water, see Percy F. Frankland ("The Removal of Micro-organisms from Water," 'Roy. Soc. Proc.,' 1885).

† It may be remarked that the pathogenic bacteria, from the nature of the case, are less adapted for life in media poor in organic materials than are the saprophytic forms, and especially those known as "aquatic."

free from pathogenic germs capable of living, any more than it is to suppose that water in the form of ice, snow, hail, or rain is incapable of conveying infection during times of epidemics; and that, in point of fact, *any* water whatever may convey living pathogenic germs from one place to another.

(7.) That the periods through which pathogenic bacteria can live in water vary according to a long series of circumstances, depending especially on the nature and vigour of the germs, whether they form spores or not, the chemical and bacteriological nature of the contents of the water, the mode of contamination, and the temperature.

(8.) That in all ordinary waters the rule is that the pathogenic forms die out sooner or later, with or without previous temporary multiplication; very commonly this final result is reached in three stages: (*a*) a preliminary diminution, due to the death of large numbers occasioned by the shocks induced by their altered environment; (*b*) a longer or shorter period of more or less active growth and multiplication; and (*c*) gradual diminution in numbers and vigour, as the available food materials become exhausted.

(9.) As regards specific forms of pathogenic bacteria, existing information extends chiefly to the following:—*

Spirillum choleræ asiaticæ has been shown to live, and even multiply, in drinking waters, though the results as to time are very conflicting; whereas some found it dead after a couple of days, others state that it lives a year† in such waters. It is impossible to reconcile all the statements; the only points of general agreement seem to be that cholera *can* be conveyed by water, and that it is, as a rule, not very resistant towards the competing forms.

Bacillus typhosus.—This seems to be much more resistant than the cholera spirillum in most cases. Meade Bolton pointed out that it needs far less organic material than cholera for its successful propagation in water. The results of several observers point to its being able to retain its powers for at least three months in drinking or river waters; but it seems to be eliminated more rapidly at higher temperatures (above 18—20° C.) than at moderately low (8—12° C.) ones. It may certainly be regarded as more able to hold its own against the resident forms in bad waters than is the cholera spirillum, and some results even suggest that the presence of other forms favours it (Hueppe, Hochstetter). Karlinski's and Holz's researches, however, are decidedly opposed to this.‡

* See Appendix C for tabulated results.

† Wolffhügel and Riedel found the cholera bacillus alive in some cases after from seven months to a year. Hochstetter gives 267—392 days. Pfeiffer has similar results.

‡ See especially "Unters. über das Verhalten der Typhus-bacillen in typhösen Dejektionen" ('Cent. f. Bakt.,' 1889, vol. 6, p. 65, and especially p. 75), and Max

Bacillus anthracis.—The vegetative rodlets of this form are invariably found to be less able to hold their own than the spores, a result quite intelligible from what is known of this well-studied Schizomycete. All agree as to the general fact that the spores of anthrax may live in sterile water for months without injury, provided the temperature is not too high.

The *Streptococcus of erysipelas* appears to be remarkably susceptible to immersion in water; it was found to be almost immediately destroyed in distilled water, and survived only five days in sterile sewage and drinking water.

Micrococcus tetragenus, according to Straus and Dubarry, can maintain itself for 18—30 days in various waters, whilst Meade Bolton gives it a much shorter lease of life.

Bacillus tuberculosis lived for more than 115 days in distilled water, and 95 in river water, according to Straus and Dubarry. Cornil* kept it alive in Seine water for 70 days.

Staphylococcus pyogenes aureus is said to live for more than 19 days in river water (Straus and Dubarry).

The same observers give more than 50 days for the bacillus of glanders, 30 days for the micrococcus of fowl-cholera, 17 days for the bacillus of swine-plague, and 20 days for that of mouse-septicæmia. Ferrari† found this form alive for several weeks in distilled water.

Appendix A.

The Literature which concerns the several Questions treated of in this Report.

We have added a short note on the scope and importance of some of the works, so far as it appears useful to do so; at the same time, it should be borne in mind that the relative value of any particular paper may depend on many circumstances incidental to the particular purpose the reader has in view, and our opinion is only intended to express what we regard as the chief feature of the work from the points of view in this Report.

Holz, "Exp. Unters. über den Nachweis der Typhus-bacillen" ('Zeitsch. f. Hyg.,' vol. 8, 1890, p. 143).

* Quoted by Woodhead, 'Bacteria and their Products,' p. 211.

† "Ueber das Verhalten von pathogen. Mikroorg., &c." ('Centralbl. f. Bakt.,' 1888, vol. 4, p. 744).

I *Literature dealing more especially with Bacteriology in general.*

GENERAL TREATISES.

Baumgarten. Lehrbuch der pathologischen Mykologie. Brunswick, 1886–90.

A treatise on the relations of micro-organisms to disease.

Cornil and Babes. Les Bactéries. Paris, 1890.

Pathological.

Crookshank. Manual of Bacteriology. 3rd ed. London.

Chiefly pathological.

De Bary. Comp. Morph. and Biology of Fungi, &c. Oxford, 1887.

Botanical.

—— Lectures on Bacteria. 2nd ed. Oxford, 1887.

Botanical. An excellent popular *résumé* of the subject.

Eisenberg. Bakteriologische Diagnostik. Leipzig, 1891.

A technical treatise designed to facilitate the recognition of the forms observed.

Flügge. Die Mikroorganismen. Leipzig, 1886. Engl. ed. London, 1890.

A concise, yet comprehensive, standard treatise for the recognition of forms, especially pathological.

Fraenkel. Text Book of Bacteriology. (Tr. Lindsley.) New York. Wood and Co.

An excellent general treatise in small compass.

Hueppe. Die Methoden der Bakterien-Forschung. Ed. 1892.

A generally-acknowledged authority on methods.

Macé. Traité pratique de Bactériologie. 1892.

A useful treatise.

Miquel. Manuel pratique d'Analyse Bact. des Eaux. Gauthier-Villars. Paris, 1891.

A work mainly advocating the dilution method of culture, and full of excellent hints.

Saccardo. Sylloge Fungorum. Vol. 8. 1889.

The standard work on the systematic classification.

Marshall Ward. The article "*Schizomycetes*" in Encyclopædia Britannica. 9th ed.

A summary of the morphology and physiology of forms, and of the general aspects of bacteriology.

Woodhead. Bacteria and their Products. London, 1891.

Résumé.

Zopf. Die Spaltpilze. 3rd ed. Breslau, 1885.

The best text-book from the point of view of polymorphy.

FOR BACTERIOLOGICAL METHODS SEE ALSO :—

Bolkin. Isolirung anaërober Bakterien. Chem. Centr. 1891. Vol. 1. No. 4.
Technical.

Brefeld. Methoden zur Unters. der Pilze. Abh. der Phys. Med. Gesell. in Würzburg. 1874.

—— Landwirthsch. Jahrb. Vol. 4. H. 1.

—— Unters. über Schimmelpilze. H. 4. 1881.
Purely technical.

Esmarch. Ueber eine Modification des Koch'schen Plattenverfahrens. Zeitsch. f. Hygiene. Vol. 1. P. 293.
Important addition to bacteriological methods.

Koch. Method of Gelatine Plate Culture. Quart. Journ. Micr. Sci. October, 1881.
Résumé.

—— Zur Untersuchung von pathogenen Organismen. Mitth. aus d. K. Gesundheitsamte. Vol. 1. Berlin, 1881. Pp. 1–48.
The classical paper on the subject of cultures on solid media.

—— Berl. Klin. Wochenschr. 1882. No. 15.
Résumé.

Petri. Kleine Modification des Koch'schen Plattenverfahrens. Centralb. f. Bakt. Vol. 1. P. 279.
A note.

Sell. Ueber Wasseranalyse unter besonderer Berücksichtigung der im Kais. Gesundh'amte üblichen Methoden. Mitth. K. Ges'amte, Berlin. Vol. 1. 1881. Pp. 360–77.
Technical.

2. *Literature dealing specially with the Bacteria of Water.*

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We now append Lists (Appendix B) of all the species of Schizomycetes detected in various kinds of waters, so far as we have been able to get at the records.

The question of synonymy is, in the present state of bacteriology, a very difficult one; we have, in most cases, placed the best known name first, but those who prefer to adopt other names may be referred to Saccardo's 'Sylloge Fungorum,' vol. 8, 1890, for the synonyms and descriptions of most of the species. Some of those recorded here are new, having been published since the above work appeared; in the case of these forms, the descriptions will be found in the memoirs quoted.

Appendix B.—Schizomycetes found in Drinking Water.

Species.	Synonyms.	Authorities for habitat.	Remarks.
Micrococcus aërogenes (Miller)..	..	Tils, 'Zeit. f. Hyg.,' 1890, p. 282	In alimentary canal
M. agilis (All-Coh.)...........	*Neisseria agilis* (Trev.)	Ali-Cohen, in 'Centralb. f. Bakt.,' 1889, vol. 6, p. 36, and Podrowsky, *idem*, vol. 10, p. 566	
M. aquatilis (Bolt)............	..	Meade Bolton, in 'Zeitschr. f. Hyg.,' 1886, p. 94	Very common in water
M. aurantiacus (Cohn)	*Pediococcus aurantiacus* (Trev.), *Bacteridium aurantiacum* (Schröt.)	Adametz, 'Die Bakt. des Nutz- u. Trinkwassers,' Wien, 1888; Podrowsky, 'Cent. f. Bakt.,' vol. 10, p. 566; Migula, in 'Cent. f. Bakt.,' vol. 8, 1890, p. 357; and Tils, 'Zeitschr. f. Hyg.,' 1890, p. 282	Common in soil and decomposition and in water
M. candicans (Fl.)	? not *Neisseria Franklandiorum*	Migula, *loc. cit.*, and Tils, 'Zeit. f. Hyg.,' 1890, p. 282	Common in atmospheric dust, &c.
M. candidus (Cohn)...........	..	Tils, *loc. cit.*	Also on potatoes
M. carneus (Zimm.)	..	Zimmermann, *loc. cit.*	
M. cerasinus siccus (List.)......	..	G. Roux, 'Analyse Microbiologique de l'Eau,' Paris, 1892, p. 285	
M. cereus albus (Schröt.).......	*Staphylococcus cereus* (Trev.), *St. cereus albus* (Passet)	Tils, *loc. cit.*	In pus, not pathogenic
M. cinnabareus (Fl.)..........	*Streptococcus cinnabareus* (Fl.)	Migula, *loc. cit.*, and Podrowsky, *loc. cit.*	Was not found in clear mountain or running streams
M. citreus (List.)	..	Roux, *loc. cit.*, p. 285	
M. concentricus (Zimm.).......	..	Zimmermann, "Die Bakterien unserer Trink- u. Nutzwässer," in 11th 'Ber. der Naturw. Gesell. zu Chemnitz,' 1890	

M. coronatus (Fl.)	*Streptococcus coronatus* (Fl.)	Migula, *loc. cit.*	Also in air
M. cremoides (Zimm.)	..	Zimmermann, *loc. cit.*	
M. cyaneus (Schröt.)	*Bacteridium cyaneum* (Schröt.)	Roux, *loc. cit.*, p. 287	Also on potatoes
M. ferritosus (Adam.)	..	Adametz and Wichmann, in 'Mitth. Oesterr. Vers.-Stat. f. Brauerei,' &c., Wien, 1888, p. 29, and Tils, *loc. cit.*	Common in water
M. fulvus (Cohn)	*Staphylococcus fulvus* (Cohn)	Roux, *loc. cit.*, p. 287	Ordinary habitat horse-dung
M. flavus desidens (Fl.)	*Streptococcus desidens* (Trev.)	Adametz, Migula and Tils, *loc. cit.*	Common in cultures and air
M. flavus liquefaciens (Fl.)	*M. flavus* (Trev.)	Migula and Tils, *loc. cit.*; also Podrowsky, 'Cent. f. Bakt.,' vol. 10, p. 566	Occasional
M. flavus tardigradus (Fl.)	*M. tardigradus* (Trev.)	Migula, *loc. cit.*	Occasional
M. fuscus (Masch.)	..	G. Roux, 'Anal. microbiol. des Eaux,' p. 280	Probably identical with *Bacillus prodigiosus*, *q.v.*
M. luteus (Cohn)	*Bacteridium luteum* (Schröt.)	Migula and Tils, *loc. cit.*, and Adametz, 'Unters. über die niederen Pilze,' &c., 1887, p. 9	Common in soil
M. plumosus (Braüt.)	..	Eisenberg, *loc. cit.*, p. 56; Roux, *loc. cit.*, p. 292	
M. prodigiosus (Ehrenb.)	(See *Bacillus*)	Roux, *loc. cit.*, p. 281	
M. radiatus (Fl.)	*Streptococcus radiatus* (Fl.)	Adametz, Migula, *loc. cit.*, and Karlinski, 'Arch. f. Hyg.,' 1889, p. 113	Occasional in cultures
M. rosettaceus (Zimm.)	..	Zimmermann, *loc. cit.*	
M. ureæ (Cohn)	*Streptococcus ureæ* (Fl.), '*Torule ammoniacale*" (Pasteur)	Migula and Tils, *loc. cit.*	Common in urine and in all waters contaminated by it
M. versicolor (Fl.)	..	Migula and Tils, *loc. cit.*	One of the commonest species in soil, air, and water, even on mountains
M. violaceus (Cohn)	*Streptococcus violaceus* (Cohn), *Bacteridium violaceum* (Schröt.), *Chromococcus violaceus* (Berg.)	Roux, *loc. cit.*, p. 289, and Eisenberg, *loc. cit.*, p. 42	Found also on potatoes
M. viticulosus (Fl.)	..	Migula and Karlinski, *loc. cit.*	
Diplococcus luteus (Adam.)	*Neisseria lutea* (Adam.)	Tils, *loc. cit.*	Soil and water

Schizomycetes found in Drinking Water—*continued.*

Species.	Synonyms.	Authorities for habitat.	Remarks.
Streptococcus albus (Masch.) ...	..	Roux, *loc. cit.*, p. 283	
St. vermiformis (Masch.)	..	Roux, *loc. cit.*, p. 284	
Sarcina alba (Eisenb.)	..	Roux, *loc. cit.*, p. 295	Common also on skin and in air
S. aurantiaca (Fl.)	..	Migula, *loc. cit.*	In air also
S. candida (Lindn.)	..	Lindner, *loc. cit.*	Brewery waters and air
S. lutea (Fl.)	..	Flügge, Adametz, Maschek, Zimmermann, Migula and Tils, *loc. cit.*	Everywhere
Pediococcus albus (Lindn.)	.. ..	Lindner in 'Bot. Centralbl.,' 1888, p. 99	
Coccus A of Fontin	..	Roux, *loc. cit.*, p. 284	
Cocco stellato of Maschek	..	Roux, *loc. cit.*, p. 288	
Coccus ruber		Roux, *loc. cit.*, p. 288	This appears to be the *M. carneus* of Zimmermann (Roux)
Bacillus aerophilus (Lib.)	..	Roux, *loc. cit.*, p. 296	Common in air
B. acidi lactici (Zopf)	*Bacterium acidi lactici* (Zopf)	Tils, *loc. cit.*	Rare in water, common in milk
Bacillus Adametzi (Trev.)	"*Brauner pigment-bildender Wasser-bacillus*" (Adametz et Wichm.)	Adam. and Wichm., in 'Mitth. Oester. Vers.-Stat. f. Brauerei,' &c., Wien, 1888, p. 51	Cultivated from water
B. albus (Eis.)	"*Weisser-bacillus*" of Eisenberg	Roux, *loc. cit.*, p. 346	
B. albus putidus (Masch.)	..	Roux, *loc. cit.*, p. 309	
B. arborescens (Frankl.)	..	Frankland, in 'Zeitschr. f. Hyg.,' vol. 6, 1889, p. 373, and Tils, *loc. cit.*	In Thames, &c.
B. aquatilis (Trev.)	"Bacille de l'eau" of Babes	Frankl., *loc. cit.*, and Cornil and Babes, 'Les Bact.,' p. 167	Common in Kent waters
B. aquatilis sulcatus (Weichs.) ..	..	Weichselbaum, in 'Das Oesterr. Sanitätswesen,' 1889, Nos. 14—23	The author describes *five* varieties of this "false typhus bacillus"

B. aureus (Adam.)	..	Roux, *loc. cit.*, p. 337; Eisenberg, *loc. cit.*, p. 140	Also found on skin of eczema patients (Eisenberg)
B. aurantiacus (Frankl.)	..	Frankl., *loc. cit.*	Kent water
B. aurantius (Trev.)	"*Orange-rother Wasser-bacillus*" of Adametz	Adam. & Wichm., in 'Oester. Vers.-Stat. f. Brauerei,' &c., 1888, p. 50	In water and air
B. berolinensis indicus (Claess.)	..	Claessen, in 'Centr. f. Bakt., 1890, p. 13	
B. brunneus (Adam.)	..	Roux, *loc. cit.*, p. 339	
B. butyricus (Hueppe)	..	Roux, *loc. cit.*, p. 310	
B. cloacæ (Jord.)	..	Jordan, in 'Exp. Invest. by the State Board of Massachusetts, 1890'	Common in St. Lawrence
B. circularis (Jord.)	..	Jordan, *loc. cit.*	
B. cæruleus (All. Smith)	..	Allen Smith, in 'Med. News,' 1887, p. 758	Schuylkill river
B. constrictus (Zimm.)	..	Zimmermann, *loc. cit.*	
B. cuticularis (Tils)	..	Tils, *loc. cit.*	New species isolated from Freiburg water
B. delicatulatus (Jord.)	..	Jordan, *loc. cit.*	
B. dendriticus (Uffred.)	..	Roux, *loc. cit*	
B. devorans (Zimm.)	..	Zimmermann, *loc. cit.*	
B. erythrosporus (Eidam)	..	Migula, *loc. cit.*	Usually in contaminated water. Absent from running streams
B. flavo-coriaceus (Adam.)	..	Adam. and Wichm., *loc. cit.*	Only in water
B. fluorescens (Trev.)	*B. Pagliani* (Trev.), "*Fluorescirender Bacillus*" of Eisenberg	Saccardo, 'Syll. Fung.,' vol. 8, p. 980	Water, soil, and even milk
B. fluorescens aureus (Zimm.)	..	Zimmermann, *loc. cit.*	
B. fluorescens liquefaciens (Fl.)	*B. fluorescens* (Trev.), *B. viridi-luteus* (Trev.), "*Grüngelber Bacillus*" of Eisenberg	Migula and Tils, *loc. cit.*; also Podrowsky, *loc. cit.*	A putrefaction form, but occurs in water, and even ice and snow
B. fluorescens longus (Zimm.)	..	Zimmermann, *loc. cit.*	
B. fluorescens non-liquefaciens (Eis.)	*B. aquatilis fluorescens* (Lust.)	Roux, *loc. cit.*, p. 341	

Schizomycetes found in Drinking Water—*continued.*

Species.	Synonyms.	Authorities for habitat.	Remarks.
B. fluorescens putidus (Fl.)	*Bacillus putidus* (Trev.), *B. Trimethylamine* (Bey.)	Migula and Tils, *loc. cit.*	Not in clear running streams
B. fluorescens tenuis (Zimm.)	..	Zimmermann, *loc. cit.*	
B. filiformis (Tils)	..	Tils, *loc. cit.*	In Freiburg waters
B. fulvus (Zimm.)	..	Zimmermann, *loc. cit.*	
B. fuscus (Fl.)	*B. brunneus* (Schröt.), *Bacterium brunneum* (Schröt.)	Migula, *loc. cit.*	Hitherto on cooked vegetables, &c. We have failed to discover whether this is the one described by Zimmermann (*loc. cit.*)
B. gazoformans (Eis.)	"*Gasbildender Bacillus*" of Eisenberg	Eisenberg, *loc. cit.*, p. 107	
B. geton (Trev.)	"Bacille de l'eau" (*b*) (Babes)	Cornil and Babes, 'Les Bact.,' p. 167	In water only
B. glaucus (Masch.)	..	Roux, *loc. cit.*, p. 300	
B. gracilis (Zimm.)	..	Zimmermann, *loc. cit.*	
B. guttatus (Zimm.)	..	Zimmermann, *loc. cit.*	
B. helvolus (Zimm.)	..	Zimmermann, *loc. cit.*	
B. hyalinus (Jord.)	..	Jordan, *loc. cit.*	
B. hydrocharis (Trev.)	"Bacille de l'eau" (*c*) (Babes)	Cornil and Babes, *loc. cit.*	Common in water
B. implexus (Zimm.)	..	Zimmermann, *loc. cit.*	
B. latericius (Adam.)	"*Ziegel-rother Wasser-bacillus*" (Adam.), *B. erythræus* (Trev.)	G. Roux, *loc. cit.*, p. 343; Adam. and Wichm., *loc. cit.*	In water only
B. janthinus (Fl.)	*Bacillus violaceus* (Schröt.), *Bacterium janthinum* (Zopf), *Chromobacterium violaceum* (Bergowz.)	Tils, *loc. cit.*, and Frankl., *loc. cit.*, and Podrowsky, *loc. cit.*	A common putrefactive form in dirty waters, &c.
B. lactis viscosus (Adam.)	..	Adametz, in 'Berliner Landwirthsch. Jahrb.,' 1891	In water of Vienna

B. lividus (Pl. and Pr.)	..	Plagge and Proskauer, 'Zeitschr. f. Hyg.,' vol. 2, p. 463, and Roux, *loc. cit.*, p. 303	
B. Lineola (Trev.)	*Bacterium Lineola* (Muell.), *Vibrio Lineola* (Muell.), *Bacterium nitrosum* (Maggi)	Aradas, in 'Atti dell' Accad. in Catania,' 1888, p. 1	Common in stagnant water
B. liodermos (Fl.)	..	Tils, *loc. cit.*	Hitherto on potatoes
B. liquefaciens (Eis.)	..	Eisenberg, *loc cit.*	
B. luteus (Fl.)	..	Migula and Tils, *loc. cit.*	Very common in putrefactions
B. liquidus (Frankl.)	..	Frankland, *loc. cit.*	In Thames
B. megaterium (De By.)	..	Tils, *loc. cit.*	Hitherto in vegetable infusions only
B. membranaceus amethystinus (Eis.)	..	Eisenberg, *loc. cit.*, p. 421	
B. mesentericus fuscus (Fl.)	*B. mesentericus* (Trev.)	Tils and Migula, *loc. cit.*	Atmospheric dust. Not found in running streams
B. mesentericus ruber (Globig)	..	Roux, *loc. cit.*, p. 321.	On potatoes also
B. mesentericus vulgatus (Fl.)	*B. vulgatus* (Trev.)	Migula and Tils, *loc. cit.*	On potatoes. Not in clear running streams
B. multipediculus (Fl.)	..	Migula, *loc. cit.*	On potatoes also
B. muscoides (Libor)	*Cornilia muscoides* (Libor.)	Tils, *loc. cit.*; Roux, *loc. cit.*	Known in cheese
B. mycoides (Fl.)	"*Erdebacillus*" (Fl.), "*Wurzelbacillus*" (Eisenb.), *B. radificans* (Pod.)	Tils, *loc. cit.*, and Podrowsky	Common in earth, air, &c. (N.B. Podrowsky)
B. nubilus (Frankl.)	..	Frankland, *loc. cit.*, and Tils, *loc. cit.*	London water
B. ochraceus (Zimm.)	..	Fazio, 'I Microbi delle Acque Minerali,' 1888, and G. Roux, *loc. cit.*, p. 103. Zimmermann, *loc. cit.*	
B. liquefaciens (Doy.)	..	Podrowsky, *loc. cit.*	In urine, if Podrowsky refers to this form. Eisenberg, *loc. cit.*, p. 112, also has a species of this name
B. plicatus (Zimm.)	..	Zimmermann, *loc. cit.*	

Schizomycetes found in Drinking Water—*continued.*

Species.	Synonyms.	Authorities for habitat.	Remarks.
B. punctatus (Zimm.)	..	Zimmermann, *loc. cit.*	
B. prodigiosus (Cohn)	..	Migula, *loc. cit.;* Tils, *loc. cit.*	Not in clear running or mountain streams. Common
B. putrificus coli (Fl.)	*B. albuminis* (Schröt.), *B. diaphthirus* (Trev.)	Tils, *loc. cit.*	Hitherto in human excrement
B. pyocyaneus (Gessard)	*B. æruginosus* (Trev.), *Micrococcus pyocyaneus* (Gess.)	Tils, *loc. cit.*	Hitherto in pus
B. pyocyaneus (β)	..	Tils, *loc. cit.*	In Freiburg waters
B. radiatus aquatilis (Zimm.)	..	Zimmermann, *loc. cit.*	
B. ramosus (Eis.)	"*Wurzel-bacillus*" of Eisenberg	Roux, *loc. cit.;* Eisenberg, *loc. cit.*, p. 126	? Same as Flügge's *B. ramosus liquefaciens*
B. reticularis (Jord.)	..	Jordan, *loc. cit.*	
B. ramosus liquefaciens (Fl.)	*B. Praussnitzii* (Trev.)	Migula, *loc. cit.* (Frankland, *loc. cit.* ?)	An aërial form
B. ruber (Frank)	..	Migula, *loc. cit.*	Frank found it in cooked rice
B. rubescens (Jord.)	..	Jordan, *loc. cit.*	
B. rubidus (Eis.)	..	Roux, *loc. cit.;* Eisenberg, 'Bakt. Diag.,' 1891, p. 88	
B. rubifaciens (Zimm.)	..	Zimmermann, *loc. cit.*	
B. saprogenes II (Rosenb.)	*B. telmatis* (Trev.)	Tils, *loc. cit.*	Hitherto on feet
B. subflavus (Zimm.)	..	Zimmermann, *loc. cit.*	
B. superficialis (Jord.)	..	Jordan, *loc. cit.*	
B. subtilis (Ehrenb.)	*Vibrio bacillus* (Müller), *V. subtilis* (Ehrenb.), *Metallacter Bacillus* (Perty)	Tils, *loc. cit.;* Migula, *loc. cit.;* Podrowsky, in 'Centr. für Bakt.,' vol. 10, p. 566	Ubiquitous and common in water. This is the well-known "*Hay-bacillus*"
B. stolonatus (Adam.)	..	Adam. & Wichm., *loc. cit.*	In water containing sugar.
B. syncyanus (Ehr.)	*Vitris syncyanus* (Ehr.), *V. cyanogenus* (Fuchs), *Bacterium syncyanum* (Schröt.)	Jordan, *loc. cit.*, and E. Roux, *loc. cit.*, p. 979	Hitherto known in milk

B. tremulus (Koch)	*Cornilia tremula* (Trev.)	Migula, *loc. cit.*	Not in clear running streams
B tremelloides (Tils)	..	Tils, *loc. cit.*	In Freiburg waters
B. ubiquitus (Jord.)	..	Jordan, *loc. cit.*	..
B. ureæ (Miqu.)	*Bacterium ureæ* (Leube), *Urobacillus Pasteurii* (Miqu.)	Migula, *loc. cit.*, and Tils, *loc. cit.*	Not in clear running streams. Usual in urine
B. vermicularis (Tils)	..	Tils, *loc. cit.* (? Frankl., *loc. cit.*)	In water of River Lea
B. vermiculosus (Zimm.)	..	Zimmermann, *loc. cit.*	
B. violaceus (Frankl.)	Not Schröter's form?	Roux, *loc. cit.*	See Roux and Saccardo (*loc. cit.*, p. 979, for synonymy, which is complex, for Eisenberg also gives a form, *loc. cit.*, p. 91, with this name)
B. violaceus-Laurentius (Jord.)	..	Jordan, *loc. cit.*	
B. viscosus (Frankl.)	..	Frankl., *loc. cit.*	In Kent waters
B. viridi-pallescens (Frick.)	*B. viridi-luteus* (Pagl.), "*Grün-gelber nicht verflüssigender bacillus*" (Eisenb.)	Tils, *loc. cit.*	In Freiburg waters, common soil
B. Wichmanni (Trev.)	"Gold-gelber Wasser-bacillus" (Adam.)	Adam. & Wichm., *loc. cit.*	An aërial form also
B. Zopfii (Kinth.)	..	Macé, *loc. cit.*	
Bacterium graveolens (Uffred.)	..	Tils, *loc. cit.*	An epidermal form
B. cæruleo-viride (Trev.)	"*Blau-grün-fluorescirendes Bacterium*" (Adametz)	Adam. & Wichm., *loc. cit.*	
B. luteum (List.)	..	Tils, *loc. cit.*; Saccardo, vol. 8, p. 1087	
B. rosaceum (Trev)	*B. rosaceum metalloides* (Dowdes.), *Bacillus miniaceus* (Zimm.)	Dowdeswell, in 'Ann. de Micrographie,' 1889, p. 310, and Zimmermann, *loc. cit.*	G. Roux suggests the identity of these two forms (*loc. cit.*, p. 305)
B. Termo (Muell.)	*Monas Termo* (Muell.), *Palmella infusiorium* (Ehr.), *Zooglœa Termo* (Cohn), *Bacillus Termo* (Trev.)	Aradas, *loc. cit.*	A very confused form. Macé believes that among the half dozen or so confounded under this name, there is one (*Bacillus Termo*) which may be autonomous (see E. Roux, *loc. cit.*, p. 332)
B. Zürnianum (List.)	*Bacillus Zürnianum* (List.)	Roux, *loc. cit.*, p. 360	

Schizomycetes found in Drinking Water—*continued.*

Species.	Synonyms.	Authority for habitat.	Remarks.
Proteus mirabilis (Hauser).....	*Bacillus mirabilis* (Hansen)	Podrowsky, *loc. cit.*, and Tils, *loc. cit.*	A soil and putrefactive form
P. sulphureus (Lind.)..........	*Bacillus sulphureus* (Holsch.), *Bacterium sulphureum* (Holsch.), *Bacillus sulphohydrogenus* (Miquel)	Podrowsky, *loc. cit.*; Eisenberg, *loc. cit.*, p. 89	In waters contaminated by fæces (synonymy doubtful. Eisenberg, pp. 89 and 129, gives Holschewnikoff's *Bacterium sulphureum* as a separate species)
P. vulgaris (Haus.)...........	*Bacillus Proteus* (Trev.)	Tils, *loc. cit.*	Soil and infusions
P. Zenkeri (Haus.)............	*Bacillus Zenkeri* (Haus.)	Podrowsky, *loc. cit.*, and Tils, *loc. cit.*	Infusions
Spirillum concentricum (Kitas)..	..	See Saccardo, vol. 8, pp. 1007 and 1010; Lustig, *loc. cit.*	Confused form?
Sp. rubrum (Esm.)............	..	Saccardo, *loc. cit.*, p. 1008; Esmarch, in 'Centr. f. Bakt.,' 1887, p. 225	

Schizomycetes isolated from Ice, Hail, Snow, &c.

Species.	Synonyms.	Authority for habitat.	Remarks.
Bacillus viridi-luteus (Trev.) ...	"*Grüngelber Bacillus*" of Eisenberg, *B. fluorescens liquefaciens* (Fl.)	Schmelck, 'Centr. f. Bakt.,' vol. 4, p. 544, and Bujwid, *loc. cit.*	A very common form
B. fluorescens nivalis (Schm.)...	See p. 65	Schmelck, quoted by G. Roux, *loc. cit.*, p. 300	It is not clear that this is not the same as the above
B. fluorescens putidus (Fl.)	See p. 66	Bujwid, in 'Centr. f. Bakt.,' vol. 1, 1887, p. 592	
B. janthinus (Zopf.)...........	See p. 66	Bujwid, *loc. cit.*	Bujwid also obtained nine other forms in hail

Pathogenic Schizomycetes which have been detected in Potable Waters.

Species.	Synonyms.	Authority for habitat.	Remarks.
Micrococcus Biskra (Heydenr.).	..	Eisenberg, *loc. cit.*, p. 227.	Pathogenic to man and other animals
Bacillus anthracis (Cohn)......	*Pollendera anthracis* (Trev.)	Poincaré in 'Comptes Rend.,' 1880, vol. 91, p. 179.	Must be frequently washed into rivers, &c.
B. canalis capsulatus (R. Mori)	..	Rintaro Mori, *loc. cit.*	
B. canalis parvus (R. Mori)....	..	Rintaro Mori, *loc. cit.*	
B. coli communis (Esch.).......	*Baterium coli commune* (Esch.), *Bacillus Escherichii* (Trev.)	G. Roux, *loc. cit.*, p. 384	It is subject of dispute as to the relations of this "false typhus bacillus" to the true one of Eberth

Pathogenic Schizomycetes which have been detected in Potable Waters—*continued.*

Species.	Synonyms.	Authorities for habitat.	Remarks.
B. cuniculicida (Flügge)	*Pasteurella cuniculicida* (Flügge), *Bacillus der Kaninchenseptikæmie* (Koch)	Koch ('Mitth. a. d. K. Gesundh. Amte,' vol. 1, p. 94), Rintaro Mori, *loc. cit.*	Eisenberg (*loc. cit.*, p. 276) points out that this form is probably identical with Pasteur's *B. choleræ gallinarum*
B. dysentericus (Trev.).........	*Bacillus der Dysenterie* (Klebs) ...	Aradas, *loc. cit.*	
B. hydrophilus fuscus (Sanar.) ..	..	Sanarelli, in 'Centr. f. Bakt.,' vol. 9, p. 193	Probably identical with *B. ranicida* of Ernst
B. murisepticus (Flügge)	*B. insidiosus* (Trev.), *B. murinus* (Schröt.), *Bacillus der Mäuseseptikæmie* (Koch)	Rintaro Mori, *loc. cit.*	
Bacillus of meningitis spinalis ..	..	Roux, in 'Ann. Inst. Pasteur.'	
B. pyocyaneus (Gess.)	..	Tils, *loc. cit.*	Pathogenic?
B. saprogenes II (Rosenb.).....	..	Tils, *loc. cit.*	Pathogenic?
B. tetani (Flügge)	*Tetanus-bacillus* (Nicol.), *Pacinia Nicolaievi* (Trev.)	Miquel, *loc. cit.*, p. 109	Its usual habitat is soil
B. typhosus (Eberth).........	*Vibrio typhosus* (Trev.)	Miquel, 'Manuel Pratique,' p. 115	N.B.—The earlier statements as to occurrence of this form must be received cautiously, as several forms are known to simulate it in growth, shape, size, &c.
Spirillum choleræ asiaticæ (Koch)	..	Koch, *loc. cit.*, Nicati & Rietsch, *loc. cit.*	
Staphylococcus pyogenes aureus (Rosenb.)	..	Tils, *loc. cit.*, Miquel, *loc. cit.*, p. 117; Eisenberg, *loc. cit.*, p. 221	Pathogenic to various animals

Schizomycetes found in Slow Rivers, Canals, Stagnant and Marsh Waters, &c.

Species.	Synonyms.	Authority for habitat.	Remarks.
Cladothrix dichotoma (Cohn) ...	*Cochlyothrix leptomitoides* (Corda)	Billet, 'Contrib. à l'Étude des Bact.,' Paris, 1890	Ubiquitous, though commonest in stagnant water
Crenothrix Kühniana (Rabenh.)	*Leptothrix Kühniana* (Rabenh.), *Crenothrix polyspora* (Cohn), *Hypheothrix Kühniana* (Rabenh.)	Cohn, in 'Beitr. Biol. Pflanzen,' vol. 3, Heft 1, and Giard, 'Compt. Rend.,' vol. 95, 1882, p. 247	Occurs in flowing rivers, but more characteristic of stagnant water
Sphærotilus natans (Kütz.).....	*Leptothrix natans* (Den.)	Eidam, in 'Schles. Gesell. f. Vaterl. Cultur,' 1876	Rivers contaminated by manufactures
Beggiatoa alba (Vauch.).......	*Oscillaria alba* (Vauch.), *O. dulcis* (Kütz.), *O. Raineriana* (Kütz.), *Beggiatoa dulcis* (Menegh.), *B. Raineriana* (Menegh.), *B. punctata* (Trevis), *Hygrocrocis Vandelli* (Menegh.)	Cohn, 'Hedwigia,' 1865, p. 81; Zopf, 'Sitzber. der Berlin. Akad.,' 1881; Winogradsky	Common in mud and stagnant waters, and also in thermal and sulphur springs
Bacillus aquatilis sulcatus (Weichselbaum)			
B. De Baryanus (Klein).......	..	Klein, in 'Ber. d. Deut. Bot. Ges.,' 1889, p. 65	Water in which algæ were putrefying
B. erythrosporus (Eidam)......	..	See Table	
B. Fitzianus (Zopf)...........	*B. subtilis* and *B. æthilicus* (Fitz.)	Zopf, 'Spaltpilze,' Aufl. 3, p. 161	In grass infusions
B. fusisporus (Schröt.).........	..	Saccardo, 'Syll. Fung.,' vol. 8, p. 978	In rivers contaminated with sugar-refuse, &c.
B. limosus....................	..	Klein, 'Ber. d. Deut. Bot. Ges.,' 1889, p. 65	In foul waters of Freiburg
B. Maydis (Majoc.)...........	*Bacterium Maydis* (Majoc.)	Saccardo, 'Syll. Fung.,' vol. 8, p. 976	In waters where maize, &c., is putrefying
B. macrosporus (Klein)........	..	Klein, *loc. cit.*	In Freiburg waters

Schizomycetes found in Slow Rivers, Canals, Stagnant and Marsh Waters, &c.—*continued.*

Species.	Synonyms.	Authorities for habitat.	Remarks.
B. malariæ (Klebs)	..	Klebs & Tommasi-Crudeli, in 'Arch. f. exp. Path.,' 1879	In malarial waters
B. Peroniella (Klein)	..	Klein, *loc. cit.*	In water where algæ are rotting
B. sanguineus (Schröt.)	..	Saccardo, 'Syll. Fung.,' vol. 8, p. 977.	Stagnant waters
B. Sohnsii (Klein)	..	Klein, *loc. cit.*	Stagnant waters
B. sulphureus (Holsch.)	*Bacterium sulphureum* (Holsch.), *Bacillus sulphydragenus* (Miqu.)	Saccardo, *loc. cit.*, p. 978 (and N.B. Table)	In cloacal waters
B. Sphinx (Trev.)	*Cornilia Sphinx* (Trev.), "Bacillus mit mehreren seitlichen Sporen" (Koch)	Cohn, in 'Beitr. Biol. Pflanz.,' vol. 2, p. 423	Stagnant waters
B. subtilis (Ehrenb.)	..	..	All over
B. stolonatus (Adam.)	..	Adam. & Wichm., *loc. cit.*	In waters contaminated with sugar-refuse, &c.
B. thermophilus (Miqu.)	..	See Table	In waters contaminated with fæces
B. tremulus (Koch)	*Cornilia tremula* (Koch)	Saccardo, *loc. cit.*, p, 1002	Putrefying infusions
B. Hanseni (Rassm.)	*Cornilia Hanseni* (Rassm.)	Saccardo, *loc. cit.*, p. 1001	In water contaminated with malt, &c.
B. ulna (Cohn)	..		
B. virens (Van Tiegh.)	..	Van Tieghem, in 'Bull. Soc. Bot. France,' 1880, p. 175	Among aquatic plants
B. janthinus	..		

Schizomycetes found in Slow Rivers, Canals, Stagnant, and Marsh Waters, &c.—*continued.*

Species.	Synonyms.	Authorities for habitat.	Remarks.
Spirillum amyliferum (V. Tiegh)	..	G. Roux, *loc. cit.*, p. 860	With *Leuconostoc*
Sp. concentricum (Kit.)	..	'Mitth. Öst. Vers.-Stat. Brau.' Wien, 1888, p. 63	
Sp. jenense (Ehrenb.)..........	*Ophidomonas jenensis* (Ehrenb.)	Saccardo, *loc. cit.*, p. 1012	Stagnant waters
Sp. Kochii (Trev.)............	*Spirochæte Kochii* (Trev.), "*Spirochaete des Wollsteiners Sees*" (Koch)	Cohn, 'Beiträge,' vol. 2, p. 420	In the Wollstein lake
Sp. leucomelanum (Perty)	*Sp. volutans* var. (Rabenh.)	Saccardo, *loc. cit.*, p. 1012	In water with rotting algæ
Sp. musculus (Mühlh.)	..	Mühlhäuser, in Virchow's 'Arch. f. path. Anat.,' &c., vol. 97, p. 96	Doubtful species; in stagnant water
Sp. pisciculus (Mühlh.)........	*Sp. nitrosum* (Maggi)	Mühlh., *loc. cit.*	With the last; common
Sp. plicatile (Ehrenb.)	*Spirulina plicatilis* (Cohn), *Spirochæte plicatilis* (Ehrenb.), *Spirillum Portæ* (Mantez.)	Saccardo, *loc. cit.*, p. 1006	Common in stagnant waters
Sp. propellens (Mühlh.)........	..	Mühlh., *loc. cit.*	Common in stagnant waters
Sp. rosaceum (Klein)	..	Klein, in 'Qu. Jl. Micr. Science,' 1875, p. 581	In fæcal waters
Sp. rufum (Perty)	..	Saccardo, *loc. cit.*, p. 1011	Stagnant waters
Sp. serpens (Muell.)	*Vibrio serpens* (Muell.)	Winter, in Rabenh., 'Die Pilze,' p. 63	Very common in stagnant waters
Sp. tenue (Erhenb.)	..	Saccardo, *loc. cit.*, p. 1009	Ponds, &c; common
Sp. undula (Muell.)	*Vibrio undula* (Muell.), *V. prolifera* (Ehrenb.)	Saccardo, *loc. cit.*, p. 1009	Very common in stagnant waters
Sp. volutans (Ehrenb.)	*Vibrio spirillum* (Muell.), *Melanella spirillum* (Bory.)	Saccardo, *loc. cit.*, p. 1012	Common everywhere in stagnating waters
Vibrio aureus (Weib.)	*Spirillum aureum* (Weib.)	Weibel, in 'Centr. f. Bakt.,' 1838, vol. 4, p. 260	In canal mud
V. flavescens (Weib.)..........	*Spirillum flavescens* (Weib.)	Weibel, *loc. cit.*	In canal mud

Schizomycetes found in Slow Rivers, Canals, Stagnant and Marsh Waters, &c.—*continued.*

Species.	Synonyms.	Authority for habitat.	Remarks.
V. flavus (Weib.)	*Spirillum flavum* (Weib.)	Weibel, *loc. cit.*	In canal mud
V. rugula (Muell.)	*Spirillum rugula* (Winter), *Melanella flexuosa* (Bory.)	Saccardo, *op. cit.*, p. 1005	Stagnant waters
V. saprophiles (Weib.)	*Spirillum saprophilum* (Trev.)	Weibel, *loc. cit.*	In infusions of vegetable matter. (Three varieties)
Spiromonas Cohnii (Warm.)	..	Warming, 'Om nogle Daum. Bakt.,' p. 370	In foul water
Sp. volubilis (Perty)	..	Saccardo, *loc. cit.*, p. 1015	Stagnant water
Bacterium enchelys (Ehrenb.)	*Bacillus enchelys* (Trev.)	Saccardo, *loc. cit.*, p. 1023	In Neva and dirty waters in Russia
B. catenula (Dujard.)	*B. catenula* (Trev.)	Saccardo, *loc. cit.*, p. 1024	Ponds and stagnant waters
B. tremulans (Ehrenb.)	*Vibrio tremulans* (Ehrenb.), *Bacillus tremulans* (Trev.), *B. nitrosus* (Mnggi)	Saccardo, *loc. cit.*, p. 1023	Common in ponds and stagnant pools
B. punctum (Muell.)	*Monas punctum* (Muell.), *Melanella monadina* (Bory.), *Bacterium tremulans* (Cohn), *Bacillus punctum* (Trev.)	Saccardo, *loc. cit.*, p. 1023	Common in ponds and stagnant pools
Bacterium cyaneo-fuscus (Bey.)	..	Beyerinck, in 'Bot. Zeitg.,' 1891, No. 43	
B. Lineola (Muell.)	..		
B. merismopedioides (Zopf)	..		
B. Termo (Duj.)	..		
Clostrydium butyricum (Prazm.)	"*Vibrion butyrique*" (Pasteur), *Bacillus amylobacter* (Van Tiegh.), *Bacterium navicula* (Reinke)	Saccardo, *loc. cit.*, p. 1008	In decaying plants and saccharine refuse
Proteus vulgaris (Hans.)	..		

Sarcina hyalina (Winter)	*Lampropedia hyalina* (Ehrenb.), *Merismopedia hyalina* (Kütz.), *Gonium hyalinum* (Ehrenb.), *Pediococcus hyalinus* (Trev.)	Saccardo, *loc. cit.*, p. 1048	Not uncommon in stagnant water
S. Reitenbachii (Winter)	*Pediococcus Reitenbachii* (Trev.), *Merismopedium Reitenbachii* (Caspary)	Saccardo, *loc. cit.*, p. 1048	In vegetable infusions, and, occasionally, in fresh waters
Merismopedia violacea (Kütz.)	*Pediococcus violaceus* (Trev.), *Agmenellum violaceum* (Breb.)	Saccardo, *loc. cit.*, p. 1048	Not uncommon in stagnant water
S. paludosa (Schröt.)	..	Schröt., 'Pilz. Schles.,' p. 153	In waters contaminated with sugar refuse
S. candida (Lindner)	..	Lindner, in 'Bot. Centr.,' 1888, p. 99	In aquarium
Leuconostoc mesenterioides (Cienk.)	*Ascococcus mesenterioides* (Cienk.), *A. Mendesii* (Van Tiegh.)	Cienkowski, in 'Arb. der Naturf. Gesell. Univ. Charkoff,' 1878, vol. 12	In waters of sugar manufactories
Micrococcus crepusculum (Ehrb.)	..		
M. agilis (Ali-Cohn)	..		
Myconostoc gregarium (Cohn)	..	Cohn, in 'Beitr. Biol. Pflanz.,' vol. 1, Heft 3, p. 183	In stagnant waters

Schizomycetes which have been found in Sea Water.

Species.	Synonyms.	Authorities for habitat.	Remarks.
Beggiatoa Cohnii (Trev.)	*B. alba* var. *marina* (Cohn)	Cohn, in 'Hedwigia,' 1865, p. 82	In aquaria
B. mirabilis (Cohn)	..	Cohn, *loc. cit.*	In aquaria
B. pellucida (Cohn)	..	Cohn, *loc. cit.*	In aquaria
B. minima (Warm.)	..	Warming, *loc. cit.*	Coast of Denmark
B. Meyeniana (Trev.)	*Oscillatoria* (Meyen)	See Saccardo, p. 938	In Atlantic. Doubtful species
B. lanugo (Ag.)	*Leptomitus lanugo* (Ag.)	See Saccardo, p. 938	French coast. Doubtful species
Phragmidothrix multiseptata (Engl.)	..	Saccardo, p. 935	

Schizomycetes which have been found in Sea Water—*continued*.

Species.	Synonyms.	Authorities for habitat.	Remarks.
Agonium centrale (Oerst.)	..	Saccardo, p. 939	
Cladothrix intricata (Russ.)	..	Russell, 'Zeitschr. f. Hyg.,' vol. 11, 1891, p. 165	
Leptothrix mucor (Oerst.)	*Leptotrichia mucor* (Oerst.), *Beggiatoa Oerstedii* (Rab.), *B. mucor* (Trev.)	Rabenhorst, 'Flor. Alg. Europ.,' vol. 2 p. 94	
L. radians (Kütz.)	*Leptotrichia radians* (Kütz.)	Rabenhorst, *loc. cit.*, p. 74	
Leptothrix spissa (Rab.)	*L. spissa* (Rab.)	Rabenhorst, *loc. cit.*, p. 74	
Bacterium Balbiani (Bill.)	*Bacillus Balbiani* (Trev.)	Billet, 'Comptes Rend.,' vol. 107, 1888, p. 423	
B. griseum (Warm.)	*Micrococcus griseus* (Winter), *Bacillus griseus* (Trev.)	Warming., *loc. cit.*	
B. littoreum (Warm.)	*Bacillus littoreus* (Trev.)	Warming, *loc. cit.*	
B. microtis (Trev.)	*Bacillus microtis* (Trev.), *Bacterium microsporum* (Trev.)		
B. fusiformis (Warm.)	*Mantegazzæa fusiformis* (Warm.)		
Kurthia Laminariæ (Billet)	*Bacterium Laminariæ* (Billet), *Billetia Laminariæ* (Trev.)	Billet, 'Comptes Rend.,' vol. 106, 1888, p. 293	
Photobacterium luminosum (Beyer.)	*Vibrio luminosus* (Beyer.), *Bacillus luminosus* (Trev.)	Beyerinck, 'Arch. Néerland.,' vol. 23, 1889, p. 403	Phosphorescent in North Sea
Ph. Fischeri (Beyer.)	*Bacillus Fischeri* (Trev.)	Beyerinck, *loc. cit.*	Phosphorescent in Baltic
Ph. indicum (Fischer)	*Bacillus phosphorescens* (Fischer), *Pasteurella phosphorescens* (Fischer)	Beyerinck, *loc. cit.*	West Indies
Ph. phosphorescens (Cohn)	*Bacterium phosphorescens* (Herm.), *Bacillus Hermesi* (Trev.)	Beyerinck, *loc. cit.*	Common phosphorescent forms of the sea
Ph. Pflügeri (Ludw.)	..	Beyerinck, *loc. cit.*	
Ph. balticum (Beyer.)	..	'Versl. en Med. d. K. Akad. Amsterdam,' 1890, p. 239	Phosphorescent in Baltic

Schizomycetes which have been found in Sea Water—*continued.*

Species.	Synonyms.	Authorities for habitat.	Remarks.
Bacillus cyaneophosphorescens (Kütz.)	..	Kütz., in 'Centr. f. Bakt.,' 1891, vol. 9, p. 158	In sea near Sydney.
B. smaragdinophosphorescens (Kütz.)	..	Ditto	Ditto
B. argenteophosphorescens (Kütz.) (var. I, II, and III, and *liquefaciens*)	..	Ditto	Ditto
Streptococcus phosphoreus (Cohn)	*Micrococcus phosphoreus* (Cohn), *M. lucens* (Van Tiegh.), *M. Pflügeri* (Ludw.), *Bacterium lucens* (Nuesch), *Anthrobacterium Pflügeri* (De By.), *Photobacterium phosphorescens* (Beyer.)	Beyerinck, 1889, *loc. cit.*; Saccardo, 'Syll. Fung.,' vol. 8, 1890, p. 1064	Common on rotting fish
Bacillus granulosus (Russ.)	..		
B. halophilus (Russ.)	..		
B. limosus (Russ.)	..	Russell, *loc. cit.*	
B. litoralis (Russ.)	..		
B. thalassophilus (Russ.)......	..		
Spirillum attenuatum (Warm.)..	..	Warming, *loc. cit.*	
S. giganteum (Warm.)	*Spirochæte giganteum* (Warm.)	Warming, *loc. cit.*	
S. marinum (Russ.)............	..	Russell, *loc. cit.*	
Spirillum sanguineum (Ehr.) ...	*Ophidomonas sanguinea* (Ehr.), *Thiospirillum sanguineum* (Winogr.)	Cohn, 'Beiträge,' &c., vol. 1, Heft 3, p. 169	A sulphobacterium
S. violaceum (Warm.)	..	Warming, *loc. cit.*	
S. volutans (Ehr.)	*Vibriospirillum* (Müll.), *Melanella spirillum* (Bory.)	Warming, *loc. cit.*	A very common form. It is Warming's var. *robustum* which occurs in sea

Schizomycetes which have been found in Sea Water—*continued.*

Species.	Synonyms.	Authority for habitat.	Remarks.
Monas Mülleri (Warm.)	..	Warming, *loc. cit.*	
Sarcina littoralis (Oerst.)......	*Lampropedia littoralis* (Oerst.), *Pediococcus littoralis* (Trev.) *Erythroconis littoralis* (Oerst.), *Sarcina Morrhuæ* (Farl.), *Coniothecium Bertherandi* (Miqu.)	See Saccardo, *loc. cit.*, p. 1049.	On crabs, &c.

Schizomycetes found in Brackish Waters.

Species.	Synonyms.	Authority for habitat.	Remarks.
Spirillum attenuatum (Warm.) . *S. leucomelaneum* (Perty.) ...... *S. Rosenbergii* (Warm.)........ *S. sanguineum* (Warm.)........ *S. violaceum* (Warm.)..........	..	Warming., *loc. cit.*	

Schizomycetes found in Ferruginous Waters.

Species.	Synonyms.	Authority for habitat.	Remarks.
Crenothrix Kühniana (Rab.) ..	*C. polyspora* (Cohn), *Leptothrix Kühniana* (Rab.), *Hypeothrix Kühniana* (Rab.)	Winogradsky	
Cladothrix dichotoma			
Leptothrix ochracea (Kütz.)			
Sphærotilus ochraceus (Bréb.)			
Lampropedia ochracea (Trev.)			

Mineral Waters.

Species.	Synonyms.	Authority for habitat.	Remarks.
Bacillus ochraceus	..	Fazio, 'I Microbi delle Acque minerali'	
B. liquefaciens			
Micrococcus candicans candidus			
Bacterium chlorinum (Engel.) ..	..	..	Fazio says *Bacil. virens*, V. Tiegh.

Schizomycetes found in Sulphurous Waters.

Species.	Synonyms.	Authority for habitat.	Remarks.
Beggiatoa alba (Vauch.)	See p. 255		
B. leptomitiformis (Mengh.)	*Oscillaria leptomitiformis* (Mengh.)	See Saccardo, p. 937	
Clathrocystis roseo-persicina			
Leptotrichia nivea (Rab.)	*Beggiatoa nivea* (Rab.), *Leptonema niveum* (Rab.), *Symphyothrix nivea* (Brügg.), *Thiothrix nivea* (Winogr.)	Winogradsky, 'Beitr.'	
L. tenuis (Winogr.)	*Thiothrix tenuis* (Winogr.)	Winogradsky, *loc. cit.*	
L. tenuissima (Winogr.)	*Thiothrix tenuissima* (Winogr.)	Winogradsky, *loc. cit.*	
Thiodictyon elegans (Winogr.) ..	..	Winogradsky, *loc. cit.*	
T. Winogradskyi (Trev.)	..	Winogradsky, *loc. cit.*	
Mantegazzaea Winogradskyi (Trev.)	*Rhabdochromatium fusiforme* (Winogr.)	Winogradsky, *loc. cit.*	
M. rosea (Cohn)	*Rhabdomonas rosea* (Cohn), *Bacterium roseum* (Trev.), *Rhabdochromatium roseum* (Winogr.)*	Winogradsky, *loc. cit.*	* Possibly forms of *Clathrocystis roseo-persicina.* See also Saccardo, p. 943
M. minor (Winogr.)	*Rhabdochromatium minus* (Winogr.)*	Winogradsky, *loc. cit.*	
Monas Okenii (Ehr.)	*Bacterium Okenii* (Ehr.), *Chromatium Okenii* (Perty)	Winogradsky, *loc. cit.*	
Monas vinosa (Cohn)	*Bacterium vinosum* (Cohn), *Chromatium vinosum* (Winogr.), *Bacillus vinosus* (Trev.)		
Spirillum volutans (Ehr.)	See p. 257	Saccardo, *loc. cit.*, p. 1012	
Bacterium Weissii (Perty)	*Chromatium Weissii* (Perty), *Bacillus Weissii* (Trev.)	Winogradsky, *loc. cit.*	
B. minus (Winogr.)	*Chromatium minus* (Winogr.), *Bacillus minor* (Trev.)	Winogradsky, *loc. cit.*	Possibly mere forms of *Monas Okenii*, &c. See Saccardo, p. 1027

B. minutissimum (Winogr.).....	*Chromatium minutissimum* (Winogr.), *Bacillus minutissimus* (Trev.)	Winogradsky, *loc. cit.*	Possibly mere forms of *Monas Okenii*, &c. See Saccardo, p. 1027
Cenomesia albida (Trev.)....... *C. lilacina* (Trev.)............	..	See Saccardo, *loc. cit.*, p. 1040	
Thyocystis violacea (Winogr.)... *T. rufa* (Winogr.)............ *Thiothece gelatinosa* (Winogr.).. *Amœbobacter roseus* (Winogr.).. *A. bacillosus* (Winogr.)........ *A. granulum* (Winogr.)........ *Thiopolycoccus ruber* (Winogr.).	..	Winogradsky, *loc. cit.*	
Sarcina rosea (Schröt.)........	*Sarcina sulphurata* (Winogr.)	Winogradsky, in 'Bot. Zeit.,' 1887, Nos. 31—37	
Lampropedia rosea (Winogr.).. *Thiocapsa roseo-persicina* (Winogr.)	*Pediococcus roseus* (Trev.), *Thiopedia rosea* (Winogr.)	Winogradsky, *loc. cit.*	

Schizomycetes found in Thermal Springs.

Species.	Synonyms.	Authorities for habitat.	Remarks.
Sphærotilus thermalis (Kütz.) ..	*Merizomyria aponina* (Kütz.)	See Saccardo, *loc. cit.*, p. 927	
S. lacteus (Kütz.)	..		
Detoniella lutea (Kütz.)	*Hypeothrix lutea* (Kütz.), *Leptothrix lutea* (Rab.), *Leptotrichia lutea* (Trev.)	Saccardo, *loc. cit.*, p. 929	
Beggiatoa arachnoida (Ag.)....	*Oscillarea arachnoidea* (Ag.), *O. versatilis* (Kütz.), *Beggiatoa versatilis* (Trev.)	Saccardo, *loc. cit.*, p. 937	
B. iridescens (Menengh.).......	*Oscillaria iridescens* (Kütz)		
B. alba (Vauch.).............	See p. 255		
B. leptomitiformis (Menengh.)..	..		
Ascococcus thermophilus (Hansg.)	..	Hansgirg, in 'Oestor. Bot. Zeitschr.,' 1888, p. 5	Saccardo says a variety of *Ascococcus Billrothii* (Cohn), *loc. cit.*, p. 1039
Micrococcus thermophilus (Hansg.)	*Micrococcus Neufvillei* (Trev.)		

Unidentified forms in Water.*

Species.	Synonyms.	Authorities for habitat.	Remarks.
Violetter liquefying bacillus	..	Eisenberg, *loc. cit.*	
Weisser streptococcus	..		
Wurmformiger streptococcus	..		
Verflussigender brauner bacillus	..		
Weisser bacillus (Maschek)	..		
Kartoffel-bacillus	..		
Citron-gelber bacillus	..	Tils, *loc. cit.*	
Gold-gelber bacillus	..		
Crème-farbiger micrococcus	..		
Fleisch farbiger bacillus (nov. sp.)	..		
Perlschnur-bacillus	..		
Pigment producirender bacillus	..		
Gelber grün-fluorescirender bacillus	..	Karlinski, in 'Arch. f. Hyg.,' 1889, p. 113	
Weisser bacillus	..		
Gelber verflüssigender bacillus	..		
Weiss-gelblicher bacillus	..		

* *I.e.*, forms as to the synonymy of which we are doubtful.

Finally we append a tabular statement showing the duration of life note that the experiments quoted have been made under very be compared closely. Nevertheless, it has appeared advisable to in this connexion.

APPENDIX C.—Behaviour of *Bacillus*

Observers.	Temperature.	Distilled water.	Sterilised ordinary water.	Non-sterilised ordinary water.
Straus and Dubarry. (N.B., proved can form spores in distilled water)	20° C.	131 days (?)	28 days* 65 days†	..
Hochstetter	13—14° to	3 days	3 days‡	..
"	18—20°	Over 154 days (sp.)	Over 154 days‡ (sp.)	..
Hueppe........	16°	..	5 days	..
Gärtner........	12°	..	6 days	..
Meade Bolton ..	20°	Over 3 months (sp.)	Over 3 months (sp.)	..
" ..	35°	Less than 3 months (sp.)	3 months (sp.)	
" ..	35°	..	55 hours	
Naegeli	..	1 year (sp.)		
Koch	..	1 year (sp.)		
Kraus	10°·5	..	..	2—4 days
Uffelmann......	12—20°	..	..	3 months (sp.)
Karlinski	8°	..	..	1—3 days
Braem.........	..	8—12 days		
Wolffhügel and Riedel	8—10°	..	..	..
"	12—16°	..	..	..
"	30—35°	..	..	..
Frankland, P. F.	15—20°	Over 60 days (sp.)	Over 60 days (sp.)	..

of certain pathogenic forms in various waters. It is necessary to different conditions by the different observers, and that they cannot compile these results, if only to show how much remains to be done

anthracis in Water.

Foul water.	Sea water or Concentrated Salt solution.	Mineral waters.	Remarks.
			Refers to bacilli * Water of Ourcq † Pure water of Vanne
..	..	15 min. to 1 hour§	Refers to bacilli and not to spores
..	..	Over 154 days§	Spores
			‡ Berlin tap water § Seltzer water
..	..	..	Bacilli only
..	..	..	All = spores
..	..	..	" "
..	..	..	Employed three waters; viz., the Munich town supply, and two well waters
..	..	..	Drinking water
3 days	..	..	Panke water, both filtered and unfiltered
Over 15 days	..	..	It seems impossible to avoid the conclusion that there must have been spores in Wolffhügel's experiments; the bacilli were taken from cultures, not blood
Over 15 days			
Over 60 days (sp.)	..	..	Employed Grand Junction water and London sewage, both sterilised

Behaviour of Typhoid Bacillus

Observers.	Temperature.	Distilled water.	Sterilised ordinary water.	Non-sterilised ordinary water.
Hochstetter	13—20°	5 days	7 days*	..
Meade Bolton ..	20°	2—3 to 30—40, 81 days (sp.)	Over 24 days	
" ..	35°	10—14—24	Over 14 days	
Heraeus	12°	..	..	Some days
"	37°	..	..	"
Wolffhügel and Riedel	15—20°	Over 15 days	Over 20 days	
"	35°	..	..	..
Braem.........	..	188 days		
Kraus	10°·5	..	..	5—7 days,‡ no longer demonstrable on 7th day
Karlinski	8°	..	..	3—6 days
De Mattei and Stagmitta	..	..	4 days	11—14 days
Hueppe........	10—20°	..	20—30 days	..
"	16—20°	..	..	..
Straus and Dubarry	20°	30—35 days	..	32§ to 43‖ days
"	25°	69 days	..	81 days§
"	35°	27 days	..	37 days§
Freytag........	..	..	..	..
De Giaxa	..	..	..	..
Uffelmann	..	..	..	Up to 2 weeks
Maschek	18—22°	..	10—80 days	

(*B. typhosus-abdominalis*) in Water.

Foul water.	Sea water or Concentrated Salt solution.	Mineral waters.	Remarks.
..	..	5 to 12 days†	* Berlin tap water † Seltzer water
Over 10 days	..	..	Water of the River Panke
			‡ Three waters used, see above
..	..	..	Innsbruck drinking water
..	..	..	In a well into which the typhoid bacilli had been introduced
5—15 days			
Over 5—30 days			§ Water of Ourcq ‖ Water of Vanne (a more pure water)
..	5 months	..	Concentrated salt solution
..	Over 25 days	..	Sea water

Behaviour of *Spirillum*

Observers.	Temperature.	Distilled water.	Sterilised ordinary water.	Non-sterilised ordinary water.
Straus and Dubarry	20°	14 days	26 days* 39 days†	
"	35°	..	30 days*	..
Hochstetter	13—20°	24 hours to 7 days	267—382 to 392 days‡	..
Babes	..	1 day	7 days	..
Wolffhügel and Riedel	16—22°	33 days	2 days to 7 months and even a year	1 to 20 days
Frankland (P. F.)	20°	..	9 days‖	..
Nicati and Rietsch	Ordinary temp. of lab. in winter	20 days	**32—38 days††	..
Ringeling	..	..	..	..
Hueppe........	10°	Over 10 days	Up to 30 days	..
"	16—20°	Over 5—10 days		
Kraus	10°·5	1 day	..	1—2 days
Karlinski	8°	3 days	..	2—3 days
De Giaxa	16—18°	..	..	..
Freytag........	..	..	..	..
Braem	..	24 hours		
Pfeiffer........	..	..	..	7 months
Maschek.......	..	40 days	60 days	..
Gärtner........	11°	..	..	1—2 days

choleræ asiaticæ in Water.

Foul water.	Sea water or Concentrated Salt solution.	Mineral waters.	Remarks.
			* Water of Ourcq † Water of Vanne
..	..	3 hours§	‡ Berlin tap water § Seltzer water
..	..	..	Berlin tap waters
7 months	..	..	Berlin tap waters
Over 11 months¶	..	..	‖ Deep-well (Kent Co.'s water and Thames water) ¶ Sterile sewage (London)
32 days	64 days to 81 days‡‡	..	** Cale †† Canal water ‡‡ Old harbour } All sterile
37 days			
..	..	..	Wiesbaden waters
..	..	..	Three waters employed as above
..	..	..	Drinking waters of Innsbruck
..	2—4 days		
..	6—8 hours		
..	..	..	A little common salt was added to the distilled water

Behaviour of *Bacillus*

Observers.	Temperature.	Distilled water.	Sterilised ordinary water.	Non-sterilised ordinary water.
Straus and Dubarry	38°	Over 115 days	95 days*	
,,	35°	25 days	30 days*	
,,	20°	24 days	27 days*	

Behaviour of *Staphylococcus*

Straus and Dubarry	20°	4—9 days	19—21 days*	
,,	35°	13 days	15 days*	
Meade Bolton ..	20°	20—30 days	20—30 to over 340 days	..
,, ..	35°	Under 5 days	Under 5 days	..
Ferrari	16—18°	Over 5 days to several weeks		
Braem.........	..	25—50 days		

Behaviour of Bacillus of

Straus and Dubarry	20°	19 days	20 days*	

Behaviour of the Bacillus of Swine Plague

Straus and Dubarry	20°	34 days	Over 17 days*	

Behaviour of the Micrococcus of Fowl Cholera

Straus and Dubarry	35°	8 days	30 days*	
,,	20°	..	2 days*	

tuberculosis in Water.

Foul water.	Sea water or Concentrated Salt solution.	Mineral waters.	Remarks.
			* Water of Ourcq

pyogenes-aureus in Water.

.. 5—10 days	..	..	* Water of Ourcq The higher numbers refer to contaminated well water

Mouse Septicæmia in Water.

			* Water of Ourcq

(*Rothlauf, Rouget du Porc*) in Water.

			* Water of Ourcq

(*M. choleræ gallinarum*) in Water.

			* Water of Ourcq

Behaviour of the Bacillus of Rabbit Septicæmia

Observers.	Temperature.	Distilled water.	Sterilised ordinary water.	Non-sterilised ordinary water.
Hochstetter	..	30 minutes to 2 days	..	..

Behaviour of Hochstetter's

Hochstetter	..	14 days	97 days*	..

Behaviour of the Finckler-Prior

Hochstetter	..	4 hours	2 days*	
Frankland, P. F.	..	1 day	1 day*	..

Behaviour of Glanders Bacillus

Straus and Dubarry	..	57 days	..	Over 28* to over 50 days

Behaviour of *Streptococcus*

Straus and Dubarry	20°	8 to 10 days	14* to 15† days	

Behaviour of *Streptococcus*

Frankland, P. F.	20°	Less than 1 hour	5 days*	..

Behaviour of Friedländer's Bacillus

Straus and Dubarry	20°	8 days	4 to 7 days*	

(*Bacillus cuniculicida*) in Water.

Foul water.	Sea water or Concentrated Salt solution.	Mineral waters.	Remarks.
..	..	30 minutes to 1 day*	* Seltzer water

" *Bacillus α* " in Water.

..	..	20 to 60 days †	* Berlin tap water † Seltzer water

Bacillus in Water.

1 day†			* Berlin tap water * Thames and deep well water † Sterile London sewage

(*B. mallei*) in Water.

			* Water of Vanne † Water of Ourcq

pyogenes in Water.

			* Water of Ourcq † Water of Vanne

erysipelatis (Fehleisen).

2—5 days†			* Thames water † Sterile London sewage

(*B. pneumoniæ*) in Water.

			* Water of Ourcq

Behaviour of *Micrococcus*

Observers.	Temperature.	Distilled water.	Sterilised ordinary water.	Non-sterilised ordinary water.
Straus and Dubarry	20°	19 days	19 days*	
Hochstetter	10—17°	18 days	18—30 days†	..
Meade Bolton ..	20°	Less than 4 days	Less than 4 days	..
" ..	35°	"	Over 4 days	

Behaviour of *Bacillus pyocyaneus*

Straus and Dubarry	..	More than 13 days	More than 20 days* More than 73 days†	
Frankland, P. F.	20°	More than 53 days		

tetragenus in Water.

Foul water.	Sea water or Concentrated Salt solution.	Mineral waters.	Remarks.
			* Water of Ourcq † Berlin tap water ‡ Seltzer water
..	..	8—11 days‡	
..	..	..	The higher numbers refer to contaminated well water

(Green Pus) in Water.

			* Ourcq † Vanne

COMMITTEE ON COLOUR-VISION.

CONTENTS.

REPORT OF THE COMMITTEE ON COLOUR-VISION.

The Committee on Colour-Vision appointed by the Council of the Royal Society on March 20, 1890, and consisting of the following members:—The Lord Rayleigh, Sec. R.S., *Chairman;* The Lord Kelvin, Pres. R.S.; Mr. R. Brudenell Carter; Prof. A. H. Church, F.R.S.; Mr. J. Evans, Treas. R.S.; Dr. R. Farquharson, M.P.; Prof. M. Foster, Sec. R.S.; Mr. F. Galton, F.R.S.; Dr. W. Pole, F.R.S.; Sir G. G. Stokes, Bart, M.P., F.R.S.; and Captain W. de W. Abney, C.B., F.R.S., *Secretary,* now submit their Report, with Minutes of the Evidence taken.

The Committee have held 30 meetings, and have examined more than 500 individuals as to their colour-vision. They have tried various methods and apparatus, including Holmgren's wool-test with Dr. Jeaffreson's and Dr. Thomson's modifications, Lord Rayleigh's colour-mixing apparatus and that of Captain Abney, Dr. Karl Grossmann's system, the lantern devised by Mr. F. Galton, and Mr. Lovibond's tintometer. They have taken the evidence of Captain Steele, of the Board of Trade; Mr. Rosser, a private instructor in navigation; Messrs. J. J. Hanbury, A. S. H. Wadden, and Bambridge, connected with the colour-testing departments of certain railways; Captain Macnab, of the Liverpool Board of Trade; Captain Angove, of the Peninsular and Oriental Steamship Company; and the following surgeons and experts in colour-vision testing:—Mr. Priestley Smith, Mr. T. H. Bickerton, Mr. E. Nettleship, Staff-Surgeon T. J. Preston, Dr. G. Lindsay Johnson, and Dr. Edridge Green. The Committee are under great obligations to Captain Abney, not only for having officiated as Secretary, but also for his very considerable labour in the determination of colour-constants, the registration of colours, and the examination, by spectral methods, of particular cases of defective colour-vision.

After weighing the evidence which they have obtained, the Committee have unanimously agreed upon the following recommendations:—

1. That the Board of Trade, or some other central authority, should schedule certain employments in the mercantile marine and on railways, the filling of which by persons whose vision is defective either for colour or form, or who are ignorant of the names of colours, would involve danger to life and property.
2. That the proper testing, both for colour and form, of al candidates for such employments should be compulsory.

3. That the testing should be entrusted to examiners certificated by the central authority.
4. That the test for colour-vision should be that of Holmgren, the sets of wools being approved by the central authority before use, especially as to the correctness of the three test colours, and also of the confusion colours. If the test be satisfactorily passed, it should be followed by the candidate being required to name without hesitation the colours which are employed as signals or lights, and also white light.
5. That the tests for form should be those of Snellen, and that they should be carried out as laid down in Appendix VI. It would probably, in most cases, suffice if half normal vision in each eye were required.
6. That a candidate rejected for any of the specified employments should have a right of appeal to an expert approved by the central authority, whose decision should be final.
7. That a candidate who is rejected for naming colours wrongly, but who has been proved to possess normal colour-vision, should be allowed to be re-examined after a proper interval of time.
8. That a certificate of the candidate's colour-vision and form-vision according to the appointed tests, and his capacity for naming the signal colours, should be given by the examiner; and that a schedule of persons examined, showing the results, together with the nature of the employments for which examinations were held, should be sent annually to the central authority.
9. That every third year, or oftener, persons filling the scheduled employments should be examined for form-vision.
10. That the tests in use, and the mode of conducting examinations at the different testing stations, should be inspected periodically by a scientific expert, appointed for that purpose by the central authority.
11. That the colours used for lights on board ship, and for lamp signals on railways, should, so far as possible, be uniform, and that glasses of the same colour as the green and red sealed pattern glasses of the Royal Navy, should be generally adopted.
12. That in case of judicial inquiries as to collisions or accidents, witnesses giving evidence as to the nature or position of coloured signals or lights should be themselves tested for colour- and form-vision.

(Signed) RAYLEIGH,
Chairman.

April 28, 1892.

The reasons on which the Committee have based these recommendations are set forth in the following pages.

Introductory

The subject of colour-sense and its imperfections is one which is necessarily of great scientific interest; but it also has a practical importance, as it affects a definite proportion of the men who are engaged in the two great industries of railway traffic and of navigation. Amongst railway men, at least, if not also amongst sailors, a suspicion has been excited that the methods adopted for testing colour-sense are not entirely trustworthy, and have had the effect of excluding some individuals from employments, the duties of which they were well qualified to discharge. On this ground alone, if on no other, it has seemed advisable to the Committee that the reasons for their recommendations should be so stated as to be intelligible, as far as possible, to all those who are interested in the matter.

Every colour, and among colours for convenience sake are included black and white, can be defined by three qualities:—1st, its hue—thus we talk of red, green, violet; 2nd, its purity, or the measure of its freedom from admixture with white—which is expressed by such terms as "deep" or "pale;" and 3rd, its brightness or luminosity—thus we say a colour is "bright," or "dark." Two colours are identical only when they can be defined as possessing the same three colour qualities, or constants as they are called, and if they differ in any one they are no longer the same. When two objects are compared together for colour, the large majority of persons will agree as to their identity or difference. Their verbal descriptions of the difference may vary slightly, but practical tests show that in reality they recognize the same variations, and hence their vision is termed *normal vision*. There is, however, not an inconsiderable minority, as will presently be shown, whose perception of colour differs very widely from that of the majority, and, for want of a better term, members of this minority are called "*colour-blind*." By this term it is not intended to convey the idea that there is absolute insensibility of vision, or even of colour-vision, but merely that the ordinary distinction between certain colours is defective The variations in the amount of this deficiency in colour-perception are numerous, and when small, are often exceedingly difficult to classify.

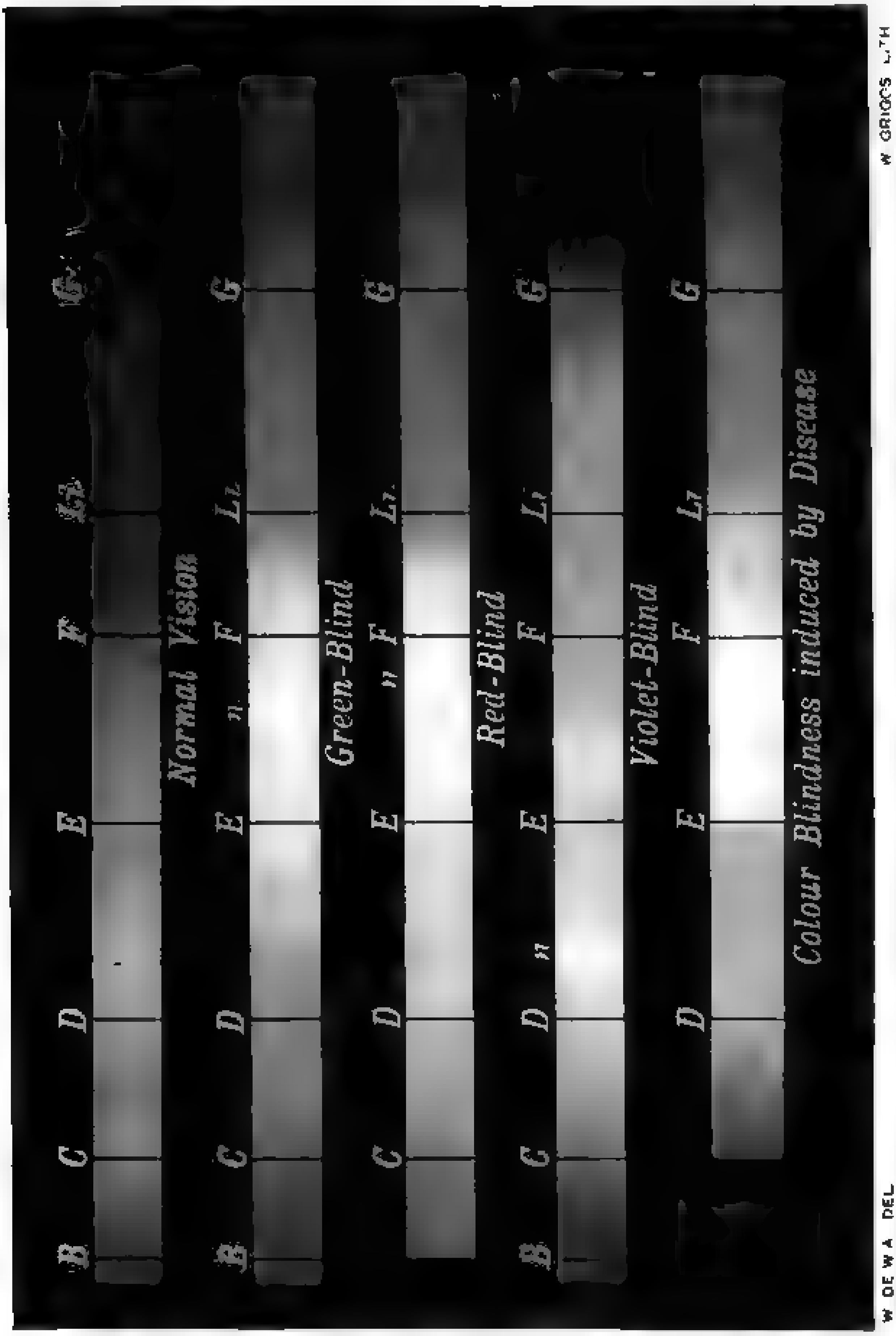
We have to regard these deviations from normal vision more from a practical than from a theoretical standpoint, and in testing for them we have to take the broad view that the colour-blindness which has to be detected is that which may be dangerous to the public in the industries already mentioned.

Character of colour-blindness.

There are some few people who fail to distinguish blue from green, and others, equally few, who only see in monochrome, but the colour-blindness which is most common, and, therefore, most dangerous, is the so-called *red-green blindness*, in which there is a failure to distinguish between red and

green; that is to say, a red-green blind person will regard a certain hue of green as identical in colour with some hue of red, another of green as identical with white, and some will also fail to see red at all of another particular hue. When it is considered that on our railways white, green, and red lights are used as safety and danger signals at night, and that the same colours are not unfrequently used for a similar purpose by day, it is very obvious that to place persons who are red-green blind in positions where the colours ought to be correctly recognised may be the cause of disasters. The same objection to the employment of persons with defective colour-vision applies also to navigation, for at night the presence of a green or red light on the port or starboard side indicates the course that a vessel is taking, and if either those in charge, or on the look-out, are colour-blind, serious risks of collisions are run.

scription of spectrum. It is proposed to enter somewhat minutely into the characteristics of red-green blindness, showing how it may be divided into two species. For this purpose it is necessary to appeal to the spectrum. When a thin slice of white light falls on one or more prisms, or on what is known as a diffraction grating, it is decomposed into a parti-coloured band which we call the spectrum, the principal colours, as given by Newton, being red, orange, yellow, green, blue, indigo and violet. If the light be that from the sun innumerable black lines will be seen interrupting this series of colours, some more marked than others. It is found that these lines always occupy the same position as regards the colour in which they are situated, and hence the more pronounced ones will act to the spectrum as milestones do to a road. Different coloured rays have different lengths of undulations in the all-pervading medium which is called ether, and the *wave lengths* of the coloured rays which, if present, would occupy the place of the principal black lines have, notwithstanding their minuteness, been determined with extreme accuracy, and this enables the position of any particular hue of spectrum colour to be numerically fixed by a reference to the wave lengths of these lines. We have said that the principal spectrum colours are those stated above, but it must be understood that they are only fully recognized by persons possessing normal vision; for the spectrum would be described by a colour-blind person in very different terms. For instance, some red-green blind would say that the red, orange, and yellow were all yellow; red would be described as dark yellow, orange as less dark, and yellow as bright yellow, whilst the green part of the spectrum bordering on the yellow would be described as yellow diluted with white. In the pure green would be pointed out a white or grey band, and the blue-green would be described as blue diluted with white; whilst the blue would be called light blue, and the violet dark blue (*see* No. 2, Plate I). Others, again, whilst similarly describing the blue and violet part of the spectrum would substitute green



No 1

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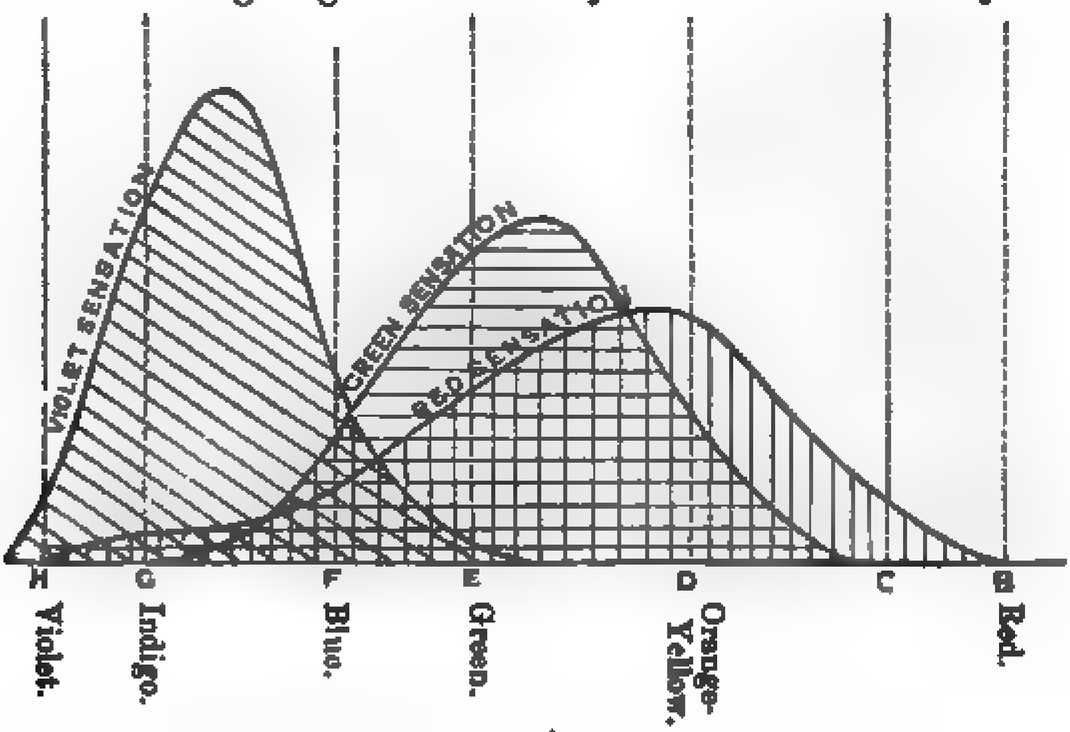
for yellow in the above description of red, orange, yellow, and yellow-green, the brightest red would be called dark green, and they would fail to see at all in the extreme red, the spectrum being shortened. These latter would also recognize a white or grey band, but it would be in a position rather nearer the blue of the spectrum than in the first case (*see* No. 3, Plate I). It is needless to say that to normal vision this white or grey band is non-existent, and whenever a person under examination sees such a band the evidence is conclusive that he is colour-blind. These differing descriptions of the spectrum show that this form of colour-blindness may be divided into two classes, which for convenience sake may be termed green- and red-blindness. Another point of difference between them is the part of the spectrum that appears brightest. To the normal eye it is the yellow, and to the green-blind it is nearly at the same place, but to the red-blind it is the green. This, perhaps, may give a clue to the designation of the spectrum colours by these two classes. To the green-blind, red and yellow are the same colour, but the yellow being the brighter he looks on red as degraded or darkened yellow. On the other hand, to the red-blind green is brighter than yellow or orange, and these appear as degraded green.

Experiment has shown that every colour in nature, as seen by a normal eye, can be expressed as a mixture of three, so that normal vision is tri-chromatic. In a similar sense the more pronounced types of ordinary colour-blind vision are di-chromatic. These colour relations must be regarded as purely subjective, for enough is now known of the nature of light to exclude the possibility of a three-fold physical constitution. In the theory of Young, subsequently, and independently, brought forward and developed by Helmholtz, light is supposed to be capable of exciting three distinct primary sensations, combined in varying proportions, and dependent upon the quality of the light. As to the character of the three sensations, Young identified them with red, green, and violet; and no widely-differing choice is possible, unless upon the supposition that the primary sensations, in their purity, are quite outside the range of our experience. The yellow of the spectrum, for example, cannot be primary, for it is capable of being matched by a suitable mixture of red and green. According to this view each primary sensation is excited in some degree by almost every ray of the spectrum; but the maxima occur at different places, and the stimulation in each case diminishes in both directions, as the position of maximum is receded from.

Young-mholtz ory of our-vision.

The following diagram will convey the idea of this theory :—



The lines with the letters B, C, D, &c., below the curves indicate certain fixed lines in the solar spectrum whose wave-lengths have been determined.

The different degrees of the stimulation given to each of the three sensations by every part of the spectrum is shown in the diagram by the heights of the curves above the horizontal base line. Thus in the middle of the spectrum, near E, each of the curves is to be found of a different height, and these degrees of stimulation of the three sensations, combined together, give the sensation of spectral green. It may be remarked that, on the scale adopted, the three sensations are supposed to be equally stimulated when white light is perceived. The areas of the three curves are therefore equal, and at the places in the spectrum where the curves are of the same height, the stimulation of the sensations is also the same. At the extreme red and extreme violet of the spectrum the curves of the red and violet sensations are alone to be found, hence at those parts the sensations are simple.

According to this theory, the two types of complete red-green-blindness are attributed to the absence of either the red, or else of the green sensation, the absence of the former corresponding to red-blindness, and of the latter to green-blindness. Where the violet and green curves cut, the red-blind person will see what to him is white, and where the red and violet curves cut the green-blind will also similarly describe his sensation of colour. To the normal eye these parts of the spectrum appear as bluish-green and green, as there is a stimulation of the green and violet sensations, or of the green alone, over and above that necessary to produce with the red sensation the mixed sensation of white.

In considering the question as to how far red-green blindness can be regarded as a *mere deficiency* in colour-perception, it is important to bear in mind that, according to recent observation, considerable deviations from the normal type may occur without any approach to colour-blindness. If we imagine a di-chromatic system be derived from an abnormal tri-chromatic system by the suppression of one sensation, it will differ from a di-chromatic system similarly derived from a normal system of colour-vision.

Violet colour-blindness.

Blindness to violet, and shortening of the violet end of the spectrum, have been described, but the instances are very few. One case of apparent violet-blindness of which the Committee have cognizance answers accurately to the Young-Helmholtz theory, on the supposition that the violet sensation is absent (*see* No. 4, Plate I).

Three other cases of congenital colour-blindness investigated by the Committee deserve special mention; two (brothers) in which there was but one sensation, answering probably to the violet sensation of the Young-Helmholtz theory, and the third in which the principal sensation was a pure green with perception of white and probably a slight trace of red. As these were all cases of congenital colour-blindness, they are mentioned as in some measure confirming the theory in question (*see* Note *a*).

Hering's theory of colour-vision.

Another theory, that of Hering, starts from the observation that when we examine our own sensations of light we find that certain of these seem to be quite distinct in nature from each other, so that each is something *sui generis*, whereas we easily recognise all other colour sensations as various mixtures of these. Thus, the sensation of red and the sensation of yellow are to us quite distinct: we do not recognise anything common to the two; but orange is obviously a mixture of red and yellow. Green and blue are equally distinct from each other and from red and yellow, but in violet and purple we recognise a mixture of red and blue. White again is quite distinct from all the colours in the narrower sense of that word, and black which we must accept as a sensation, as an affection of consciousness, even if we regard it as the absence of sensation from the field of vision, is again distinct from everything else. Hence the sensations, caused by different kinds of light or by the absence of light, which thus appear to us distinct, and which we may speak of as "native" or "fundamental" sensations, are white, black, red, yellow, green, blue. Each of these seems to us to have nothing in common with any of the others, whereas in all other colours we can recognise a mixture of two or more of these.

This result of common experience suggests the idea that these fundamental sensations are the primary sensations, concerning which we are inquiring. And Hering's theory attempts to reconcile, in some such way as follows, the various facts of colour-vision with the supposition that we possess these six fundamental sensations. The six sensations readily fall into three pairs, the members of each pair having analogous relations

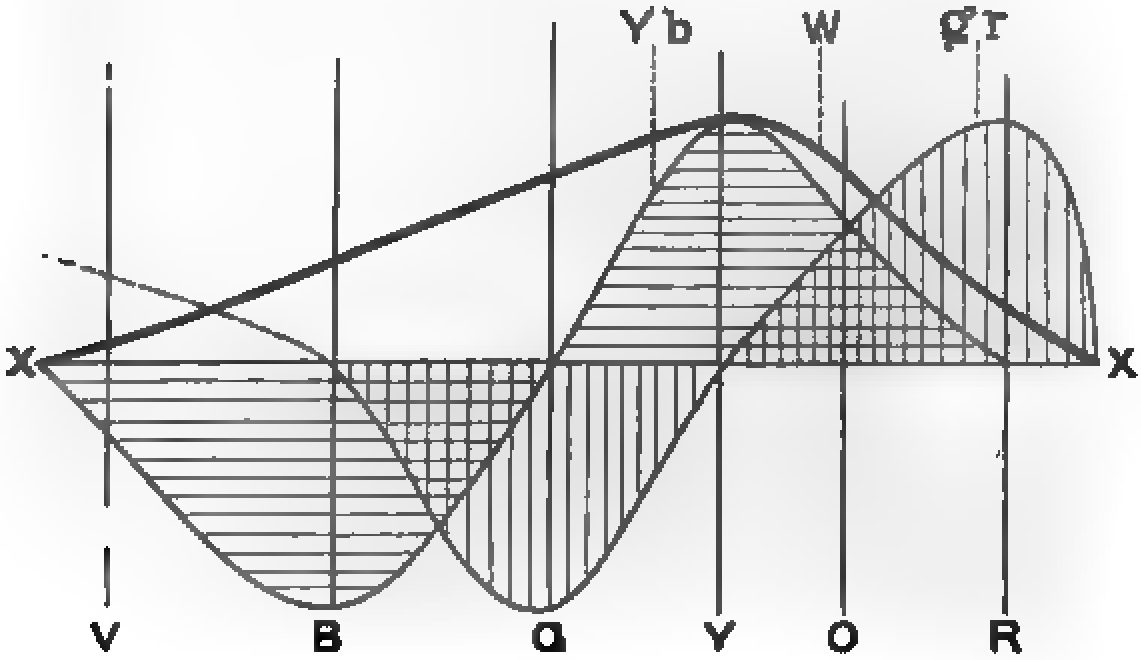
to each other. In each pair the one colour is complementary to the other; white to black, red to green, and yellow to blue.

Now, in the chemical changes undergone by living substances, we may recognise two main phases, an upward constructive phase in which matter previously not living becomes living, and a downward destructive phase in which living matter breaks down into dead or less living matter. Adopting this view we may, on the one hand, suppose that rays of light, differing in their wave-length, may affect the chemical changes of the visual substance in different ways, some promoting constructive changes (changes of assimilation), others promoting destructive changes (changes of dissimilation); and on the other hand, that the different changes in the visual substance may give rise to different sensations.

We may, for instance, suppose that there exists in the retina a visual substance of such a kind that when rays of light of certain wave-lengths—the longer ones for instance of the red side of the spectrum—fall upon it, dissimilative changes are induced or encouraged, while assimilative changes are similarly promoted by the incidence of rays of other wave-lengths, the shorter ones of the blue side. But, it must be remembered, that in dealing with sensations it is difficult to determine what part of the apparatus causes them; we may accordingly extend the above view to the whole visual apparatus, central as well as peripheral, and suppose that when rays of a certain wave-length fall upon the retina, they in some way or other, in some part or other of the visual apparatus, induce or promote dissimilative changes and so give rise to a sensation of a certain kind, while rays of another wave-length similarly induce or promote assimilative changes and so give rise to a sensation of a different kind.

The hypothesis of Hering applies this view to the six fundamental sensations spoken of above, and supposes that each of the three pairs is the outcome of a particular set of dissimilative and assimilative changes. It supposes the existence of what we may call a red-green visual substance, of such a nature that so long as dissimilative and assimilative changes are in equilibrium, we experience no sensation, but that when dissimilative changes are increased, we experience a sensation of (fundamental) red, and when assimilative changes are increased we experience a sensation of (fundamental) green. A similar yellow-blue visual substance is supposed to furnish, through dissimilative changes, a yellow, through assimilative changes a blue sensation; and a white-black visual substance similarly provides for a dissimilative sensation of white and an assimilative sensation of black. The two members of each pair are therefore not only complementary but also antagonistic. Further, these substances are supposed to be of such a kind that while the white-black substance is influenced in the same way, though in different degrees, by rays along the whole range of the spectrum, the two other substances are differently influenced by rays of different wave-length. Thus, in the part of

the spectrum which we call red, the rays promote great dissimilative changes of the red-green substance with comparatively slight effect on the yellow-blue substance; hence our sensation of red.



The vertical shading represents the red and green, and the horizontal shading the yellow and blue, antagonistic pairs of sensations. The thick line indicates the curve of the white sensation.

In that part of the spectrum which we call yellow the rays effect great dissimilative changes of the yellow-blue substance, but their action on the red-green substance does not lead to an excess of either dissimilation or assimilation, this substance being neutral to them; hence our sensation of yellow. The green rays, again, promote assimilation of the red-green substance, leaving the assimilation of the yellow-blue substance equal to its dissimilation; and similarly blue rays cause assimilation of the yellow-blue substance, and leave the red-green substance neutral. Finally, at the extreme blue end of the spectrum, the rays once more provoke dissimilation of the red-green substance, and by adding red to blue give violet. When orange rays fall on the retina, there is an excess of dissimilation of both the red-green and the yellow-blue substance; when greenish-blue rays are perceived there is an excess of assimilation of both these substances; and other intermediate hues correspond to varying degrees of dissimilation or assimilation of the several visual substances.

When all the rays together fall on the retina, the red-green and yellow-blue substances remain in equilibrium, but the white-black substance undergoes great changes of dissimilation; and we say the light is white.

According to this theory what are called red and green blindness are identical. The yellow-blue and white-black sensations remain, but the red-green sensation is absent in both. The white

or grey seen in the spectrum would then be due to the white-black sensation, as it alone is stimulated at that point.* (*See* Note *b*.)

Colour-blindness caused by disease.

The kinds of colour-blindness so far alluded to are the congenital types, but there is another form of colour-blindness which is induced by disease or injury. The former is apparently by far the most common, and so far as we have ascertained, is incurable, but the latter may be induced at any period of life, and in very many cases is capable of improvement or cure.

Colour-blindness induced by disease or injury exhibits distinctive features of its own, which are not present in cases of congenital colour-blindness. It is usually confined to the central region of the retina, and the extent of the diseased area varies largely. Defective form-vision is an invariable accompaniment, and it can be usually diagnosed by the recognized tests. (For these tests *see* Appendix VI.) In several cases induced by excessive use of tobacco, as also in that induced by progressive atrophy of the optic nerve, the Committee have found in examinations made with the spectrum that the sensations of white and blue alone were perceived in the central portions of the retina. The blue seen corresponded with the blue region of the spectrum, and all other colours were described as white. In other cases, a faint yellow in the yellow portion of the spectrum was perceived together with the blue and white, as in the first-named cases (*see* No. 5, Plate I). That these sensations were rightly described is to be assumed from the fact that these persons when in health have normal vision, and also, that on healthy portions of the retina all colours stimulate the normal sensations. (*See* Appendix C.)

Statistics of colour-blindness.

The earlier statistics of defective colour-sense must be dismissed as untrustworthy, having been arrived at by various, and frequently by inaccurate methods of examination, and having, on the whole, a marked tendency to error in the direction of excess. The first on which reliance can be placed are probably those of Dr. Joy Jeffries, of Boston, U.S.A., who personally examined 19,183 male persons, mostly in educational institutions, and who found among them 802 colour-blind, or 4·12 per cent. Among 14,764 females, he found only 11 cases, or 0·0084 per cent. In 1880, the Ophthalmological Society of London appointed a Committee to inquire into the subject, and they found that amongst 14,846 males, 617 or 4·16 per cent. were colour-blind. Amongst 489 females, 0·4 per cent. were defective in colour-vision. The report of this Committee is contained in the first volume of the "Transactions" of the Society, and an extract from it will be found in Appendix I.

The Committee were furnished with some statistics regarding colour-blindness in two Japanese regiments. Out of 1,200 men examined, 19 were red-blind, 10 green-blind, 12 incompletely colour-blind, and 27 had weak colour-vision. This gives 3·4

* Without deciding between these two theories, it has been found convenient to accept the terminology of the Young-Helmholtz theory.

per cent. of soldiers who were colour-defective, without including those who are classed as having weak colour-vision. The above statistics all point to the prevalence of colour-blindness amongst the male population, and to the fact that such defects are not confined to one nationality or race. The small percentage of colour-blindness found amongst women is remarkable, but as it does not enter into the questions on which the Committee have to report, it need not be further dwelt upon.

Results and dangers of colour-blindness.

The Committee have already briefly alluded to the mistakes which congenitally colour-blind people are likely to make; but in order to emphasize it, they will enter rather more fully into the subject. In the first place, let it be remembered that to the red-blind and to the green-blind there is one green in the spectrum which they cannot distinguish from white, and which for convenience may be designated as their *neutral colour*. On the one side of this neutral band they see but green or red, more or less diluted with their neutral colour, and on the other side blue, also similarly diluted. The dilution increases as the neutral point is approached, and for some little distance on each side of it (unless a comparison with white be at hand) the dilution is so large that the colour may be mistaken for the neutral colour.

As all colours in nature, except purples, can be matched by the normal eye with some one spectrum colour (which we may call the *dominant colour*) more or less diluted with white light, we can, where the dominant spectrum colour of a signal is known, indicate in the terms used by a person possessing normal vision what each class of colour-blind would see.

Perhaps this is best shown as a tabulated statement:—

Colour of Signal.	To a Red-Blind Observer.	To a Green-Blind Observer.
Red.	Green.	Red.
Green, the dominant spectrum green being on the red side of the neutral band.	Green mixed with a large proportion of neutral colour.	Red mixed with a large proportion of neutral colour.
Green, the dominant spectrum green being at the neutral band of the red-blind.	Neutral colour.	Red mixed with a very large proportion of neutral colour.
Green, the dominant spectrum green being at the neutral band of the green-blind.	Blue mixed with a very large proportion of neutral colour.	Neutral colour.
Green, the dominant spectrum green being well on the blue side of the neutral band.	Blue mixed with a large proportion of neutral colour.	Blue mixed with a large proportion of neutral colour.
White.	Neutral colour.	Neutral colour.

The neutral colour on the Young-Helmholtz theory in the case of the red-blind, would be a peacock-green, and in that of the green-blind a purple.

The table shows that a signal exhibiting certain hues of green might be mistaken for a red one, since they both might appear to the one class green and to the other red; and that with one hue of green (differing slightly in the two cases, however) it would give the same sensation as white. In only one case, viz., that in which the dominant spectrum colour to the normal-eyed is well on the blue side of the neutral points, would the signals be distinctly different in colour.

Colours of railway signal glasses.

The following table gives the wave-length in the spectrum of the dominant colours of the signals which have been adopted by some of the principal railway companies when illuminated by (1st) a light of the whiteness of the arc electric light, which does not differ much from that of day-light, and (2nd) by gas-light. The percentage of white light mixed with the spectrum colour is also shown, together with the luminosity of the light transmitted. How closely the green signals approach to the neutral points of the completely colour-blind, when the mental standard of whiteness is that of daylight, can be well judged if it be remembered that these points lie between 5,200 and 4,900 for both types (*see* Note *c*, page 304).

	Glass.	Electric light.			Gas light.		
		Dominant wave-length in ten millionths of million (?)	Percentage of white light in colour.	Luminosity, naked light = 100.	Dominant wave-length.	Percentage of white light in colour.	Luminosity, naked light = 100.
Reds	Great Western ruby glass	6250	7	10·4	6275	12	13·1
	L.B.S.C.	6200	0	10·4	6200	0	13·0
	Great Northern	6250	0	9·0	6275	0	10·0
Greens	Great Western	4925	46	21·8	5070	50	18·1
	L.B.S.C.	4925	38	16·2	5050	34	12·5
	Great Northern	5100	61	19·2	5170	62	19·4
	Great Eastern	5000	54	15·0	5120	40	15·0
	Saxby and Farmer's, as ordinarily supplied where no special glass is ordered	4925	24	7·6	5050	22	6·9
	Bottle green glass (District Railway)	5500	32	9·1	5320	50	10·6

In a testing-room, when signal lights are used as tests, colour-blind persons may possibly be able, with practice, to name the different coloured signals correctly, recognizing them by their

relative brightness, and by their dilution with neutral colour. Thus, a bluish-green signal might be distinctly known by its blue hue, whilst if yellowish-green, it might be recognized by the neutral colour being slightly tinged with the only other spectrum colour which they see. Again, a green whose hue, whether pure or diluted with white, accurately coincides with that part of the spectrum where the neutral band is situated, might probably be mistaken for white, though, even from that, it might be distinguished by its lower luminosity. The practical tests the Committee have carried out confirm this view; men who are absolutely colour-blind having passed such a test without being detected. It might be supposed that if the colours of signals could be rightly recognized in the testing-room they would be equally well recognized elsewhere. It must, however, be recollected that the atmospheric conditions of the testing-room are often very different from those which are found outside. As a rule any judgment of the colour of a signal which depended upon its brightness would be fallacious. A dirty glass, or a misty atmosphere, would introduce a liability to error. The red signal of danger might then be mistaken for the green or white signal of safety, and *vice versâ.* It must also be remembered that a signal light, as a rule, has no white light adjacent to it with which to compare it, and thus a decision as to whether a light is neutral, or slightly coloured, has to be arrived at under great disadvantages. We shall presently call attention to the conditions which regulate the choice of the colours to be used as signals; here it is sufficient to say that, even if a green were used, whose dominant spectrum colour lay on the blue side of the neutral bands, mistakes might still occur, more particularly in certain conditions of foggy weather, when white light in its passage is deprived of the blue rays in greater proportion than the green, and the green in greater proportion than the red (*see* Note *d*, page 305).

Description of incomplete colour-blindness.

We have so far confined our attention to colour-blind vision of the dichromatic type. Incomplete colour-blindness is less likely to lead to accident than that which is complete; but any colour-blindness, in which there is approximately a neutral or grey point in the spectrum, should be regarded with great suspicion. On the other hand, there are many people who have a slightly shortened spectrum, who are yet able to distinguish all colours, and see no neutral point. These cannot be considered to be practically colour-blind. There are again others to whom the spectrum is considerably shortened, but not to the extent that it is in complete red-blindness, and they have what is apparently a neutral point in the spectrum, lying very close to that which is found in the complete colour-blind cases. The presence of this neutral colour points to such a degree of imperfection in colour sense that it must be classed as dangerously defective. A certain and prompt recognition of a green signal colour by these last would undoubtedly be difficult under some

conditions of atmosphere, or if the mind were disturbed by some imminent danger.

Colour-blindness induced by disease.

In colour-blindness, induced by disease or injury, although the loss of colour sense is usually confined to a small area of the retina, yet, as it is the central area, and therefore the part on which the image of small objects naturally falls, the danger of mistaking a colour is as great, and even more so than in congenital colour-blindness; for loss of colour-sense is in this case as already has been stated accompanied by loss of form-sense.

Colour-blind persons should be rejected for certain occupations.

On the general grounds that have been explained, the Committee are of opinion that it would, under any circumstances, be dangerous to trust the reading of signals to anyone who is totally or even partially colour-blind to the extent indicated above, and this opinion is fortified by practical tests which they have carried out. They consider that such a person under no circumstances should be allowed to take a post for which this defect renders him physically unfit, and with this object in view the tests employed in the examination should be of a nature to at once detect, not only pronounced colour-blindness but defective colour-vision of the above character.

Most suitable colours for signals, and causes which modify their selection.

On some railways white lights instead of green have been used as safety signals, but the former are liable to be confounded with other white lights which are not signals, more particularly in the neighbourhood of towns. At sea the evidence shows that the use of a second coloured light in addition to a red is a necessity, and that a white light could not be substituted for it.

It has been suggested, on theoretical grounds, that all danger of misreading signals would be avoided by using for one a red and for the other a pure blue, as each of these colours is recognized by the red-green blind. Certain difficulties, however, present themselves in practice which preclude the employment of the blue, more especially for night signals. The desiderata for signals are, that they should be as bright as possible, and that their colour should be distinct when viewed at a distance. A red glass transmits about 10 per cent. of the luminosity of the lamp-light behind it; it is also a saturated colour, and appears unaltered in hue from whatever distance it may be viewed. A blue glass, as ordinarily met with, will appear purple, or even whitish, by lamp-light, as it transmits, besides blue, a large proportion of red rays, and, if it be pale, it will also transmit a variable quantity of all the colours of the spectrum: moreover, the luminosity of the light transmitted is, at the best, only some 4 per cent. of the naked light. If two glasses, one of blue-green and another of cobalt blue, be placed together, in front of the light, the red rays will be cut off, and the light will be a fairly pure blue, but the luminosity will be reduced to about 2 per cent. When the effect of foggy weather on the carrying power of different lights is considered (*see* Note *a*, page 303), it will be understood how this small luminosity will be again diminished, and that it will become practically *nil*. In making

the selection of signal colours, these facts have to be taken into account. The choice of a red light as a signal light is one in which theory and practice really agree, and it is in the selection of a colour for a second signal that the difficulty arises. The only colour for the latter, which the red-green blind would be able with certainty to distinguish from the red, is the pure blue, and this has been shown to be an impracticable choice. This being the case, the second signal should be of the kind most suitable for normal colour-vision without regard to the requirements of those who are colour-defective. Evidently for carrying power it should be as near the brightest part of the spectrum as possible, but far enough away from the red to render the signals easily distinguishable. A yellow or greenish-yellow is inadmissible, as it might be mistaken for a white light under some circumstances, as is also the case with those greens which, when sufficiently light to be effective, allow some red rays to pass. It is for reasons such as these that most railway companies have adopted as a danger signal a rich ruby-red, and for a safety signal (where a white light is not used) a blue-green, which varies slightly in hue on different lines, as was shown in the table given at page 292.

Colours fo[r] signals adopted b[y] railways.

The sealed pattern standards of red and green glasses used in the Royal Navy are the best that have come before the Committee, and they suggest their adoption both for railways and the mercantile marine. The sealed pattern green inclines to blue and cuts off all red light. The blue-green of the spectrum, when mixed with about 25 per cent. of white light, matches the hue of this glass, and owing to this comparatively small dilution it will also appear as a fairly saturated colour. Its luminosity also approaches that of the standard red light, which is very desirable.

Standard signal glas[ses] used in R[oyal] Navy.

The direct evidence before the Committee is not sufficient to enable them to say that accidents, either by land or by water, have conclusively been traced to defective colour-vision, yet this by no means disproves the high probability that accidents have really occurred from such defects.* There can be no doubt that every colour-blind person employed afloat, or upon railways, in certain capacities, must of necessity be a source of danger to the public. As is known, colour-blindness is hereditary to a large extent, and we have it in evidence before us that in the training vessels in which the orphan children of sailors are educated there are about 4 per cent. of colour-blind boys. We may therefore take it, apart from all other evidence, that a considerable number of the fathers of these orphans who were employed as sailors must have suffered from the same defect; and we have it in direct evidence that a considerable number of colour-blind people, officers and seamen, are actually at sea at

Accidents through colour-blindness.

* In Dr. Joy Jeffries' book on "Colour-blindness; its Dangers and its Detection," the case of the loss of the "Isaac Bell" is fairly conclusively traced to colour-blindness. Other cases are mentioned in Mr. Bickerton's evidence.

the present time. Allowing for those whose colour-vision has been found defective by the inadequate tests used, and who may not be afloat, it is certain that out of the 120,000 seamen who are employed, there must be a large number who are colour-defective, and consequently a source of danger to life. The statistics of the examinations of eyesight on railways, so far as they have come before the Committee, are eminently unsatisfactory. Although candidates for employment are occasionally rejected for defective colour-vision, yet the percentages of the rejections on different railways differ widely from each other, and from the average percentage of colour-blindness of the male population. The evidence taken on this subject points to these differences being due to the variation in efficiency of the tests employed, and the Committee have been forced to the conclusion that some men, whose vision is defective for colour and for form, are in all likelihood employed in positions where normal vision is essential for public safety.

The evidence, moreover, points to the fact that steps have not hitherto been taken (at least, as a rule) in judicial inquiries relating to the causes of accidents, to ascertain whether they were due to defective vision. The Committee are strongly of opinion that in cases of collision or accident, where the evidence is conflicting as to the recognition of a coloured light, witnesses should be examined both for colour- and form-sense.

The Committee have had before them evidence regarding the colour-vision testing of the marine service as laid down by the Board of Trade.

...ard of ...ade tests ...r colour-...sion.

Tests may be divided into two classes: one dependent upon the correct naming of a colour, and the other on its correct appreciation. The first class are intended to combine with the detection of colour-blindness that of colour-ignorance, or the defective knowledge of the names of colours. The last class are intended to detect colour-blindness alone, colour-ignorance being independently tested. The tests which the Board of Trade have officially adopted, are described in Appendix II. The examination consists in requiring the examinee to name correctly the colours of cards by day-light, and of coloured glasses by lamp-light. The correct naming of the colours is alone insisted upon.

...aming ...lours, a ...fective test.

The Committee consider that the tests themselves and the method of applying them are necessarily open to very grave objection. The Board of Trade test cards and coloured glasses can be procured from dealers, and the Committee have no hesitation in saying that the colours may be correctly named in the testing room by colour-blind persons after a certain amount of instruction, which would consist in teaching them to distinguish the different cards or test glasses by their different luminosities. The glasses are red, pink, three kinds of green, yellow, neutral, standard blue, and pale blue, all of which are viewed by artificial light, usually that of an oil lamp. In trials made

before the Committee, several people, whom Holmgren's test had proved to be colour-blind, passed this lantern test, a fact sufficient to show that it is unsafe to trust to it. But besides this uncertainty as to the rejection of the colour-blind, it appears to the Committee that an injustice may also be done to the candidates by its use. They believe that a perfectly normal-eyed person, who has been educated to observe colours, would not be able to speak positively as to the precise names of the colours of some of these glasses when illuminated by lamp-light. Less educated candidates would be much more liable to make mistakes in these puzzling tints (which the Committee consider have neither use nor significance), and, from sheer confusion, to misname those colours which are the only real tests, and thus to fail to pass the examination. The only safeguard to a candidate thus rejected lies in the fact that he can be re-examined, and that more than once. Cases have been brought before the Committee's notice where a candidate who has failed at first has passed in a subsequent examination. If the test for colour-blindness used by the Board of Trade were fair to the candidate, and perfectly efficient, such a re-examination would be unnecessary, and passing upon re-examination would be impossible.

Railway companies' tests. The evidence given by representatives of various railway companies shows that very few have any adequate system of testing. Nearly all the methods employed are defective, and even where the wool-test is applied it usually breaks down from a choice of improper colours, both for standards and comparisons. In some instances, a person, whom the Committee know to have very defective colour-vision, has been passed in their presence by railway examiners as possessing normal eyesight, and the impression made on the Committee is that many have probably been passed into the service who should most certainly have been excluded.

Tests in Royal Navy. The Committee have had the opportunity of examining the different tests carried out by the Royal Navy, and are glad to find that they are most efficient, and of such a nature that it may be presumed that no one can pass them who is sufficiently defective in colour-vision to be any source of danger. The long periods over which the examination lasts, however, precludes the adoption in their entirety of these tests used for railways or the mercantile marine. The sealed pattern glasses for signals are excellent, and, as already stated, the Committee would suggest their adoption as the universal signal colours.

Tests for colour-blindness. The Committee are of opinion that the tests for colour-blindness should be of such a character that they will readily determine whether a man is or is not colour-blind, but that, except for scientific purposes, it is not necessary that they should indicate what kind of mistakes he is likely to make. The fact that a person is found to be colour-blind by an efficient test, properly applied, is amply sufficient to show that his employment in certain occupations is a danger to the public. We lay some

stress on this point, as, if it were required from the examiner that he should specify what would be the nature of a mistake that an examinee would be likely to make, it would open the door to controversy, and thus defeat the ends for which an examination is instituted. What should be required of the examiner is merely a statement that the candidate has either passed or failed in the examination. In cases of failure, where the candidate is under the impression that a mistake has been made, an appeal to some properly appointed expert should be allowed, and his decision should be final.

ests recommended by the Committee.

The Committee have carefully considered the question as to what tests should be recommended for general adoption on railways and for the marine service.

They are of opinion that tests which involve the naming of colours should be avoided in deciding the question of colour-blindness. Failure to satisfy these tests may be due to colour-ignorance, and lead to the rejection of persons who are not really colour-blind. A candidate who fails should be informed to what cause his failure is due, whether to colour-blindness or to colour-ignorance, with a view to subsequent re-examination in the latter case. On the other hand, if the objects which the examinee is required to name are few in number and accessible to the public, since the chances are that no two of them are exactly alike even to a colour-blind person, he might be instructed as to the names which he is expected to give them, and thereby persons who are really and seriously colour-blind might be passed by the examiner as being free from any defect. Besides trustworthiness, the tests should be adapted for the examination of large bodies of men, and, provided efficiency be not sacrificed, they should be of an inexpensive nature. After practical trials, and also from theoretical considerations, the Committee are of opinion that the simplest efficient test is the wool-test of Holmgren, applied either in the form which Holmgren himself recommends, or in that of Jeaffreson, which is based on precisely the same principles.

olmgren's st.

A full description of Holmgren's test, and of the proper methods of applying it, extracted from Holmgren's work on the subject, is given in Appendix III, page 375.

It is most important that the standard test-colours should be of a proper character both as to hue and also as to dilution with white, the efficiency of the test depending almost entirely on a proper selection. The Committee recommend that sealed patterns of all three test-colours should be kept by some central authority —such as the Board of Trade; and that every set of test-wools should be officially passed as fulfilling the necessary conditions as to these standard colours, and also as to the sufficiency and variety of confusion colours.

The standard test-colours which have been approved by Professor Holmgren have been referred to the spectrum. The first standard is a light green colour, which can be matched with a green in the spectrum (λ 5660), when 40 per cent. of white is added

The second standard skein is light purple or pink, and its complementary colour is a green in the spectrum λ 5100. The colour is diluted with about 40 per cent. of white. The third test-skein has a colour corresponding with a red of the spectrum (λ 6330) diluted with 18 per cent. of white.

Should an accident happen at any time to the standard sealed pattern skeins, the exact hues can be reproduced from the spectrum by a reference to these numbers. The Committee cannot conceal from themselves the fact that the wools are apt to deteriorate with use, both by the constant handling and also, to some extent, by light. In the test as carried out by Holmgren there is but little doubt that almost as much information is conveyed to the examiner by the way in which the different skeins are picked up to match the test-skein as by the absolute matching itself, and this procedure involves handling them and also exposure to light. The assortment of wools which is used in practical testing should therefore be renewed from time to time.

Jeaffreson's test. In Jeaffreson's form of this test, which is given in Appendix IV, page 392, the handling of the colours is avoided, the match being made as there described. The hesitation evinced by the colour-blind in matching the test-colour is, in this instrument, also, of great utility to the examiner; moreover, it has been found practically that as many, or even more persons can be examined in a given time by it than by the original plan. The Committee are therefore of opinion that this modification may be admitted if desired by the examiner.

These wool-tests will detect red-, green-, and violet-blindness, and all other forms of congenital defective colour-vision. The matches of colours will indicate to the examiner the character and extent of the defect.

Examination on appeal. In cases of appeal the examinations should take a wider range. The test with the spectroscope is decisive, and in Appendix V. is described a method of applying it which the Committee think may be convenient and satisfactory.

Tests to be avoided. All tests in which the wools are suspended from a bar, even though the test-skeins may be of proper colour and tone, should be avoided, since the order of arrangement might be ascertained by some means or another by those who are tested. It is quite true that the order might be changed; but in an examination of this character, where large numbers may be under trial, any frequent changing of the order would be impracticable, and hence there would be no security that the test was efficient. The same objection applies to all diagrams of colours which the examinees are required to match with standard colours. Coaching here is even more easily carried out than with the suspended wools, since the diagrams are in the market, and the tints cannot be changed in position.

Other tests. There are some other efficient tests that are less adapted for examining large bodies of men than the wool-tests, but which may be well applied to demonstrate the presence of colour-

blindness in individual cases. Those of Dr. Grossmann are a good example of this class of test. An opinion has been expressed, and with some plausibility, that the only fair tests by which to prove that a man's colour-vision renders him unfit to distinguish coloured lights or signals are the coloured lights themselves when seen under the same circumstances as those under which they would have to be observed. It has already been shown that, with practice, it may be possible for a colour-blind person to distinguish between colours by their different luminosities and dilution with white, but it has also been pointed out that such recognition would be rendered uncertain by differing states of the atmosphere and by other conditions. If it were possible to eliminate the chances of correct guessing, which would be very large when using such tests, it would be necessary that the examination should be a prolonged one, being repeated many times with differing conditions of weather. If it were not carried to this extent, it might equally well be conducted in a testing room, where the apparent size of the signals to the eye could be imitated with great exactness. But the uncertainty of this method, even when the variable factor of weather is absent, is exemplified by the results of the examination of railway employés at Swindon, conducted by the Committee. They found, as already stated (*see* Appendix VII), that several passed the lamp-test who had failed to pass the wool-test, and that some passed one lamp-test, but failed to pass another similar one on the same occasion. Had the examination of these men been to ascertain their fitness for certain employments requiring normal colour-vision, and been conducted by the lamp-test only, some would have been admitted into the service, and have been a source of danger to the public.

lour-ignorance. The Committee have had to consider whether what has been called colour-ignorance, that is, ignorance as to the names of colours, is as objectionable as colour-blindness for certain employments. The possibility of the existence of real colour-ignorance, such as would lead to a non-recognition of the true colour of a signal, appeared to them very doubtful until they had taken the evidence of Staff-Surgeon Preston, R.N.; for it was hard to conceive of ignorance which would lead to confusion in naming a red, a green, and a white signal. His evidence, however, was conclusive of its existence at certain recruiting centres, and more especially in a certain class of recruit. It may be mentioned that in the actual testing of large bodies of men by the Committee, in no case was there a trace of colour-ignorance exhibited by those possessing normal vision, unless in regard to nondescript colours. Red, green, blue, and white were always correctly named, except where the person examined was proved to be colour-deficient.

There is one type of colour-ignorance which of course may often be encountered; a foreigner on board an English-commanded vessel, would be, practically speaking, colour-ignorant if

he were unable to name the colours in English. It is in evidence before us that in navigation it is often requisite that the look-out man should, without a moment's delay, pass to the officer in charge the name of the colour of a light, and that hesitation, whether caused by true colour-ignorance or from want of knowledge of English terms, might involve disaster. This being the case, the Committee are strongly of opinion that for the marine services the examination for colour-vision should exclude not only men who are colour-blind within the limits already indicated, but also those who are colour-ignorant, whether from defective education or from want of knowledge of the English names. No man should be accepted as a look-out unless he were found capable of naming the signal colours correctly and intelligibly, and without hesitation.

Ignorance the name signal col should be bar to em ment.

Tests for colour-ignorance

The tests which the Committee recommend for the detection of colour-ignorance are very simple. After the tests for colour-blindness have been satisfactorily passed it would suffice to ask the examinees to name the reds and greens of the wool-tests, and if any hesitation was evinced to test them with a lantern-test, such as that proposed by Mr. Galton. Men rejected for colour-ignorance of either type should not be considered permanently ineligible, but only until such time as their education in the subject was perfected, for it must be recollected that, unlike colour-blindness, colour-ignorance is curable.

Re-testin the marin and railw services.

In the marine service, it appears that on each stage of promotion an officer is tested as to his colour-vision. On some railways also, on promotion, an employé's eyesight is re-tested. It does not appear that such tests are undertaken with the idea that colour-blindness of the congenital type may have become more pronounced, or may have induced it by disease, but rather with the view that those who have been previously tested may have been passed improperly. No doubt these re-examinations are a safeguard; but if the tests already passed had been such as to render detection a certainty, there would be no necessity for repetition except for the detection of such colour-blindness as may be due to disease, injury, or over-use of tobacco. Colour-blindness due to these last causes is at first very seldom appreciated by the sufferer, and is usually only discovered upon his consulting a medical man for impaired form-sense. This raises the question as to whether defective colour-sense other than congenital might not, in some cases, be found in those on whom the lives of passengers and others depend.

Tests for colour-blindness induced b disease.

Special tests for colour-blindness induced by disease will very rarely be necessary if, as should always be the case, every examination for colour-vision is preceded by one for form. These latter tests are so well known, that the Committee do not think it necessary to enumerate them. If a candidate is found to have defective form-vision of a pronounced type he certainly should be ineligible for the positions of responsibility from which colour-blind persons should be excluded, and the test for form-vision would as a rule

therefore exclude the colour-blind of this type (*see* Appendix VI). It should be remarked that it is quite possible that the Holmgren wool-test might be passed satisfactorily by colour-blind people of this type, more particularly when the diseased area is confined to a small central spot in the retina; in fact, this has happened twice in the presence of the Committee.* The Committee would therefore rely rather on the form-test being stringently carried out, than on instituting another colour-test for this particular class of colour-blindness.

rsons to be rusted h examina- n.

The qualification to be required from the examiners has received the careful consideration of the Committee. An examiner both in the railway and in the marine services would be called upon to carry out not only the tests for colour-vision but also those for form, and the Committee are of opinion that he should be required to obtain a certificate of competency from some duly constituted authority. Testing, such as we have recommended, requires careful training, and is not to be learnt except by practice, for it requires not only a registration of absolute mistakes, but also a ready observation of the manner in which the candidate acts whilst under examination. The Committee would not insist upon the examiner being a medical practitioner, but it is probable that a medical training would be of advantage. They are further of opinion that there should be a periodic inspection of the different testing stations by duly qualified ophthalmic surgeons, who should report upon the condition of the testing appliances and upon the mode in which the tests are carried out; and who might be the authorities to whom an appeal from a rejected candidate should be referred.

In no case should any test be allowed in substitution of those recommended, though supplementary tests might be tried if desired. The passing or rejection of the candidates should always be based on the tests which have been laid down.

iodic mination.

As colour-blindness of the congenital character is never acquired, it is unnecessary that any one who has already been examined for colour-vision by efficient tests should be re-examined. But as tobacco-blindness is not uncommon, the form-sense of those men whose failure in vision would be dangerous to the safety of the public should be tested periodically, say, once every three years.

sons to be mined.

The Committee are not prepared to give a list of those posts from which the colour-blind should be excluded. Pilots, look-out men and officers on board ship; engine-drivers, firemen and

* Captain Abney prepared for the Committee pellets of baked clay of about $\frac{1}{8}$ inch diameter, coated with pigments in distemper of the same hues as those of the wools in the Holmgren test. The images of these small pellets fill such a minute area of the retina that those colour-blind persons were unable to pick out from a small trayful of them correct matches to any of the standard test colours, though they were perfectly able to pick out all those coloured with any shade of blue with ease. As stated above, they passed the ordinary wool-test, the colours being readily distinguished outside the diseased central retinal area.

signal-men on railways, evidently require sight unaffected by defects in colour or form, and there may be other positions, both in the marine service and in that of railways, which should also be included. Some central authority should make a schedule of such positions, and should take measures to enforce the exclusion of colour-blind persons from them.

NOTE (*a*).

Cases of abnormal colour-blindness.

The cause of the different sensations which are conveyed to the brain is a matter which is still in doubt. It is difficult to conceive that matter which is so comparatively gross as the rods and cones which are situated on the retina can be affected by the merely mechanical action of the vibrations of light.

The little we know about the actual nature of sensations leads us rather to believe that the nervous processes which are the foundation of sensations are, like other nervous processes, the outcome of chemical changes in nervous substances. And it has been suggested that vision originates in the chemical changes of a certain substance (or substances) in the retina, that the chemical condition of this substance, which has been called visual substance, is especially affected by the incidence of light, and that the changes so induced determine the beginnings of visual impulses and thus of visual sensations. We know that light can decompose a substance by acting on its molecules, and thus induce a chemical change in it.

In photographic processes, for instance, we know that the molecules of the sensitive substance are split up by white light, and further, that when these comparatively simple substances are exposed to the spectrum, although it is found that a considerable extent of it produces chemical changes, there is one particular part which acts more strongly than the rest of it. The curve of sensitiveness exhibits the same characteristics as those of the colour sensations in the Young-Helmholtz theory. If it be conceded that the retinal substance acted upon by light is a mixture of three analogous compounds, each having a maximum sensitiveness at a different point of the spectrum, we can account for the three fundamental sensation curves shown in the diagram at page 286.

NOTE (*b*).

Any complete theory of colour-vision must account not only for normal vision and congenital colour-blindness, but also for those cases of defective colour-sense which are due to disease or injury, and which differ so widely in character from each other.

Difficulties accounting

It is somewhat difficult to see how the Young-Helmholtz theory accounts for the last species of colour-blindness. Accord-

for colour-blindness induced by disease by the Young-Helmholtz theory.

ing to this theory, the mixed sensations of red, green, and violet produce the sensation of white light; but evidently in the cases where colour is absent in every part of the spectrum except in the blue—the rest being seen as white—some different explanation is required. Or again, if we take into account the fact that at a certain distance from the centre of the retina all sensation of colour, varying according to its luminosity and its hue, is lost, though light is still seen, the ordinary application of the theory cannot be insisted upon.

Hering's theory and colour-blindness induced by disease.

It may seem that Hering's theory is fully capable of explaining most of these phenomena, but there are facts against its acceptance which are very weighty. For instance, according to this theory, the sensations of red and green, and of yellow and blue, ought always to be present together, but in some cases of colour-blindness caused by over-use of tobacco, and atrophy of the optic nerve, the blue is the only colour sensation felt, the yellow being absent from that part of the spectrum in which it should be present. Again, when the intensity of the light producing the spectrum is reduced the sensation of red disappears long before that of green, which shows that the two sensations are not always co-existent. The shortened spectrum of what are called the red-blind is also opposed to the theory, for the luminosity of the green is proportionally much greater to them than the red than it is to the green-blind.

Note (*c*).

The neutral point of the spectrum will vary in all cases of colour-blindness according to the whiteness of the light with which the spectrum is compared. Even to the normal eye there is a ray near the yellow which can match very closely indeed the light of a gas lamp or candle, though there is none which matches the whiteness of ordinary day- or sun-light. Now a match made by the normal eye of a coloured light with some ray of the spectrum will be equally a match to the colour-blind of either type, since in both the colour and its match in the spectrum the same one sensation will be absent. It therefore follows that their neutral point, with a candle or oil lamp as a standard of whiteness, must be the same yellow ray, but to the red-blind this ray would appear greenish if compared with the white of day-light, and to the green-blind reddish. If the mental picture of white light were that of day-light, then evidently the green signal light would have to be much bluer to the colour-blind than to the normal eye, to prevent a confusion between it and their neutral colour than would have to be the case when lamp-light is the mental image of white light. In testing a large number of men by lamp-light it was invariably found that its light was always called yellow or orange by the normal-eyed, and we may therefore suppose that the general idea of whiteness is derived from

Shift of the neutral point in the spectrum caused by different qualities of white light.

day-light. As this is the case with the normal-eyed, it may be assumed that the same mental standard of whiteness would be adopted by the colour-blind.

NOTE (*d*).

Effect of fog on the colour of signals.

In discussing the most suitable colour of signals, the question of the possible alteration of hue by the interposition of fog between them and the observer must be taken into account. There are white fogs and yellow fogs, the difference between the two being chiefly in the size of the particles of water, dust, or soot which are to be found in them. In a white fog away from large towns the particles are chiefly water, but whilst the great majority must be large compared with the length of a wave of light, yet some will be present which are very much smaller. In a yellow fog the fine particles are much more largely present, and the yellowness is largely due to this fact, for when particles, whose sizes are comparable to a wave-length of light, are present between the source of light and the observer, the law of scattering requires that the blue part of the spectrum of the light reaching the latter should be much more enfeebled than the green, the green than the yellow, and the yellow than the red. A blue-green signal glass will therefore appear rather less blue in a white fog, and even yellowish-green in a yellow fog, and it may happen that the loss of what are blue and green to the normal eye will shift the colour of the signal to the red side of the neutral point in the spectrum of each type of a colour-blind person, and then both red and green signals will appear of the same tint to him, though the latter will appear more diluted with the neutral colour. It follows therefore that in a fog the liability of the colour-blind to mis-read signals is very much greater than in ordinary clear weather.

EVIDENCE TAKEN BY THE COMMITTEE.

Evidence of Mr. Hanbury, of the Metropolitan Railway.

In the engine department, the men are examined as to perception of colour before they can qualify for drivers, but I think not for porters. If there is any doubt, we examine those engaged in traffic matters again, but not unless. We examine with the wool test, which I have here. We place this (a horizontal bar, from which were suspended skeins of wool about fifteen inches in length, and all bright colours) on the table in front of the man to be examined, and also a few skeins of wool, as an independent test. We ask the man what he understands by a danger signal; he says "red," naturally; then I ask him what colour represents a caution signal; he says "green." I say, can you find the colour representing the danger signal. He looks, and perhaps picks out red; if he hesitates at all in his first choice, we ask him if he is quite sure it represents the danger signal. He perhaps says it does. Then we ask as to the caution signal; also test him with regard to the skeins of wool, and request him to pair or match the colour with a similar one on the frame; and if there is any doubt, we ask him as to brown or blue. Suppose he were to take this (mauve), we should test him again. I have not found many such cases on the Metropolitan Railway. Men sometimes mis-name the colours. We do not ask him the names of colours, but ask him to match them. We also ask him to pick out the "danger" or "caution" signal colour, and we sometimes ask for the best red. We allow the man examined to make a minute examination between the colours. I cannot tell exactly how many men we have personally examined in this way, but I started my examinations in 1869, and have perhaps met with three cases of colour-blindness. I cannot give an estimate as to the number examined. The wool test is the first test which my Chief undertakes, but when going on the footplate (on the engine) I examine them again myself. Agricultural labourers as a rule answer the questions as to the colour of the signals correctly. I never heard of the engine drivers rejecting the firemen, nor the case of a man going colour-blind subsequently.

Question.—Do you have a certain proportion of men over-running the danger signal in a way which cannot be accounted for?—I know of the case of a man at King's Cross passing the danger signal—an aged man—but I found his colour-sight good. The positions of the coloured wools on the bar are not shifted. The firemen are only tested once or twice as to colour, and afterwards if promoted to be drivers. We do not test with lamps, nor as to alteration of colour by fogs. We test them with regard to other colours than red and green if there is an

evidence of the necessity. We explain that the red signal is a danger signal, but we very seldom find a man ignorant of this, they generally know something of the work. There is no test with lamps, because the glass which gives the green light is blue by day-light. It is not a signal green glass, it is a blue glass [the glass is peacock blue]. We test by day-light. If we have green glass with the lamp we find it an indifferent light. The glass we use with gas is of a very definite green.

In case of hesitation, would you ask the man for some further examination, such as to pick out wool which was not far from a given colour?—We should not pass them if there was any doubt. If a man chose the wrong colour I should not think of passing him. It rests with the examiner and not with a doctor to pass a man. Cases have occurred where men have not been able to pass the examiner's sight test and have been sent to the doctor, who has given a certificate. We have a sight test; the test is with the single eye—one being covered; also with both eyes uncovered. We do not test our men at night as well as by day-light, further than already explained.

If accidents only happen at night, should they not be tested in that respect?—No, I think not. We take it for granted if a man can tell red in day he can at night.

The CHAIRMAN: Suppose that there were wools here, none of which matched that (red) exactly, but some nearly; if you were to ask a man to pick out a near match, and he showed hesitation, would you regard him as suspicious?—If he picked out the nearest, I should consider he had answered correctly. The picking out of an exact match does not prove that a man is not colour-blind. I have seen a man pick out brown, and call it red. Red represents the danger signal, green represents caution. On the Metropolitan Railway our signals are so arranged that in the event of any breakage of the glass the white light is treated as a danger signal. [Mr. Rix was here called, and the witness applied the different tests to him, the questions being answered to the satisfaction of Mr. Hanbury, who remarked that Mr. Rix had good sight. He was, however, informed that that gentleman was colour-blind. Mr. Hanbury stated that they would pass him on the Metropolitan Railway. Mr. Rix was recalled, and his colour-blindness proved by Dr. Grossmann's test.]

The CHAIRMAN: Do you ever use Holmgren's test?—I have never seen it.

Mr. BRUDENELL CARTER: If Mr. Rix were confronted with a single light he would not be able to tell which was green. His deliberation shows he cannot do it in a moment?—I must admit that Mr Rix being colour-blind is an eye-opener. We have about 500 men engaged in machine work, or on the engines.

Mr. BRUDENELL CARTER: Could you let me test them at some time by arrangement; it would not take long?—Yes, I should be very pleased to. I do not think painters painting various colours on the carriages and other things make mistakes

in colour. If they did, it would be discovered during their apprenticeship.

(By Dr. Pole): I do not know of any other railway using blue instead of green glass. I do know whether they use pure green. I have remarked that some glasses are bluer than others. We call them a better green. They are blue in daylight, but not by night.

(*The Witness then withdrew.*)

Evidence of Mr. Wadden, of the London and South Western Railway.

The men entering the service of our Company are tested when they first enter, and again when they are promoted to be firemen, and every second year after that; and if during one of these biennial periods a man is promoted to driver, he is specially tested then. In the traffic department, every man is tested upon entering the service. They are tested in this way. I have brought the material in actual use for the purpose. These wools [the wools consisted of browns, drabs, sombre greens, one brighter green, and nondescript colours of very low tone] are placed upon a horizontal rod promiscuously, and the man asked to pick out three or four reds, blues, or greens, and if he makes an error in one of these, he is tested again with other colours. A man may have a good notion of colours, and not know what to call them. We do not find they mistake red for green. In addition to this test for the traffic department, in the locomotive department there is a night test. The room is darkened, and a box is fitted with a lamp at the back, and various coloured glasses are put in front, commencing with a small disc, perhaps the size of a pin's head, and gradually increasing till we get to one the size of a sixpence, the man being asked what colour he thinks is being shown to him. He is ten or twelve feet from the lantern. The smallest disc is the size of a pin's head, about one-eighth of an inch, or hardly as much, perhaps. We find the men are not so ready with the night test: they are more accustomed to colours by daylight, and find it easier in daylight to distinguish the colours than at night. These colours (wools) were provided by our storekeeper. I am not prepared to say under whose instruction.

Mr. Brudenell Carter: Among these there is not a single red. I should say they were selected by a colour-blind person!

Capt. Abney: I am not colour-blind, but I should not know what to call some of these.

The Witness: Our locomotive foreman says many men fail in green who do not in red. I am told some men looked at that (green) and called it red. A further test is sometimes tried by sending men to the Ophthalmic Hospital, where there is a doubt, and I have been told that the hospital authorities confirmed our examination. This wool test is what we call the daylight test,

and the night test is with the absolute colours of signals. We give some puzzling colours at night, one of which is an orange light. The tints are graduated, we only use 4 glasses, white, red, green, and orange. All the glasses are of the same intensity. We do not try to imitate fogs, we simply have the lamp at the back of the glass, and the man in front; the room being darkened. I find larger lights are more easy to distinguish than smaller ones The light behind the coloured glass is 10 or 12 candle power. It is not used with a bull's eye; it is a perfectly plain glass. Our signals are with bull's-eyes, with plain glass in front for colours. The plain glass is certainly a more severe test than with a bull's-eye, for I myself can see the flame through the colours.

Question.—If you look at the blue glasses in a lamp outside a chemist's shop, you often see the flame is red. Might not a mistake be made somewhat in this way?—I think it might confuse men not accustomed to artificial lights. I think our test a very severe one, and a large number of men fail to pass. I could hardly say what percentage. The diameter of the lights used on the lines is about 6 inches. The man can see these lights a mile away. At 300 yards with a 6-inch light across he could see to stop a train well. I am speaking approximately, of course. The test at 3 yards with the disc as large as a sixpence is about equal to the 6-inch light at the distance of 24 yards. The man is wanted to see the latter at 300 yards, but we have the smaller light, which is perhaps only $\frac{1}{8}$th of an inch. [Mr. Rix, who is colour-blind, was here called and tested by the witness, who stated that he had passed to his satisfaction.] Witness explained that with the traffic men the question is not of such importance as with drivers and firemen, who are in charge of the train and mind the brakes; the sight of the traffic men is not tested so severely as the drivers and firemen. [Witness exhibited the specimens of the glasses actually in use in the signals and in the lamps; also samples of the coloured signal flags.]

Mr. Priestley Smith's evidence was to the following effect:—

"*Acquired Colour Blindness* differs in one important respect from congenital Colour Blindness—the congenital defect is often associated with a normal form-sense, while the acquired defect is always, or almost always, associated with more or less loss of the form-sense; that is to say, a man who has once had a normal colour-sense cannot lose it without losing more or less of his form-sense. (Leber has collected several supposed exceptions to this rule, but they are not conclusive, and are doubted even by Leber himself. 'Graefe-Saemisch Hand-Book,' vol. v, p. 1037.)

"In testing the visual function it is important to distinguish between the central and the more peripheral parts of the field. The centre of the retina—the macula lutea—is the part used in looking accurately at any object. The object is seen with much

greater precision when pictured on this area than when pictured on any other part of the retina. Colour-vision becomes progressively less and less acute from the centre to the periphery of the retina.

"Hence, in considering defects of vision, it is important to distinguish between those which affect the centre of the field and those which affect the more peripheral parts. A defect involving the centre implies an impairment of colour-sense and of form-sense at the point where they are most acute.

"The form-sense at the macula lutea is tested by ascertaining what is the smallest type which can be read at a given distance—according to the principle laid down by Prof. Snellen. (Snellen's Test-types.)

"The colour sense at the macula is tested by holding a small coloured object on the end of a black wire or rod at a convenient distance in front of the patient, and moving it in such a way that its image moves across his retina from the periphery to the centre. If there is a defect at the macula the colour, instead of appearing most intense at that part, appears less intense, or is lost altogether. I commonly employ a circular piece of red sealing wax on the end of a wire. I make the patient stand with his back to a window, cover one eye with his hand, and look straight at my forehead with the other. Watching that he does not move his eye, I hold the red object before him at 30 or 40 degrees to one side of his line of vision. I ask him the colour. He says 'red.' I try again at the other side, and above, and below the line of vision, with the same result. I then move the object into his line of vision, and repeat the question. If his vision is impaired at the macula he says 'it looks brown,' or 'dull,' or 'dirty,' or 'I can't see it at all.' He has a 'central scotoma'; a central area of defective vision—an 'absolute scotoma' if vision is entirely lost in this area; a 'colour scotoma' if the object is still perceived, but not its colour. A saturated colour gives the clearest indications; a pale colour is a more delicate test for slight defects, but requires better power of observation on the part of the patient. Red is practically the most effective test. When red is lost, green is lost also. Green is said to be lost before red. I cannot speak positively of this from my own observation. In order to test this point it would be necessary to choose a green and a red of precisely equal intensity—*i.e.*, of equal white-value.

"*Central Scotoma* is caused by various affections of the optic nerve, the choroid, and the retina. I exhibit charts taken from three cases of the kind, which show the position and extent of the affected area.

"*Central Colour Scotoma due to excessive use of Tobacco*, is one of the commonest forms. I hand in some statistics which show that this condition—known as tobacco amblyopia—constitutes rather more that 1 per cent. of eye disorders in my own hospital practice; rather less that 1 per cent. in my private practice.

The scotoma has usually an oval shape, the long axis being horizontal; it includes the macula and extends as far as the optic disc.

"Persons who suffer in this way are usually what would be called heavy smokers, and they usually use strong tobacco. In a large proportion of cases there has been some mental shock or depression as an additional cause. The patient may have been a heavy smoker for many years without apparent injury; then his wife or child dies, or he loses money or employment; sleep and appetite fail, his strength is reduced, and within a few weeks the tobacco begins to take effect.

"Entire disuse of tobacco usually effects a great improvement of vision in a month or two, or even sooner; complete recovery is not uncommon. I do not know that tobacco amblyopia is commoner in seaport towns than in Birmingham. Many sailors smoke heavily, but their out-door life would probably render them less liable than the less robust inhabitants of manufacturing towns.

"In relation to the present enquiry, tobacco amblyopia is probably the most important form of acquired colour defect, for it comes on insidiously, without known cause, without pain, and without other sign of illness; it affects both eyes, and it does not prevent the man from doing rough labouring work. The patients who come to us are often still occupied in rough work; a clerk affected in like manner is quite unable to follow his occupation.

"Tobacco amblyopia would prevent a man from recognising the colour of a distant lamp. Possibly he might recognise it by viewing it indirectly, that is eccentrically, but as a matter of fact I think that such a man would always look directly at the lamp, if he could still see it at all, and would therefore fail to recognise its colour. On the other hand he would recognise the colours of large surfaces, for the retinal pictures of these would extend beyond the scotoma. I think he would recognise the colours of skeins of wool, such as are used in testing the colour sense.

"Persons suffering from tobacco amblyopia complain of bad sight; they never complain of being unable to see colours properly; they are seldom aware that they have lost the power of seeing the colour of small objects, until the fact is pointed out to them.

"*Peripheral and Eccentric Defects of Colour-sense* are common. They are present whenever the field of vision is contracted. They may co-exist with normal vision for form and colour at the centre of the retina, but in many cases central vision is impaired also. Wherever the defect be situated, the colour-sense and the form-sense are impaired simultaneously, but the sense of colour is lost before the sense of form. Green is said to be lost first of all; certainly green and red are lost before yellow and blue.

"*Neurasthenic Amblyopia* is one of the conditions in which the visual field contracts, and the colour-sense is impaired or lost. It occurs in persons suffering from nerve-exhaustion, hysteria, reflex disturbances, shock, &c.

"The function is lowered throughout the whole of the field of vision. The fields for white and the several colours are contracted or abolished. As regards area, the least active region, viz., the periphery, fails first; the most active, the centre, fails last. As regards colour-perception, the feeblest, viz., that for green, is said to fail first; the strongest, that for blue, last. I have not tested the precise order in which the different colours are lost, but I have ascertained in some cases that blue and yellow are still recognised when red and green are no longer recognised.

"These cases are characterised by undue proneness to fatigue of the visual function. The field contracts while the eye is under examination. The test is made with the registering perimeter. The limit of the field is determined in each meridian in succession, and on going round the field a second time, we find a further contraction in each meridian, and obtain a spiral line as seen in the chart exhibited. A blue glass placed before the eyes often enlarges the field and raises the acuteness of vision, presumably by cutting off the more exhausting rays. (*See* 'Ophthalmic Review,' May, 1884.)"

The practical outcome of the foregoing appears to be that a man who has once been found to have normal form-sense and normal colour-sense, need not be re-tested for colour so long as his form-sense remains normal; that is to say, if at any future time he can still read the normal line of Snellen's types, he is certainly not suffering from any acquired defect of colour-sense at the centre of the retina.

EYE DEPARTMENT.—QUEEN'S HOSPITAL.

Statistics of Tobacco Amblyopia.

Year.	Out Patients.	Tobacco Amblyopia.	Percentage.
1879	293	4	1·70
1880	357	4	1·12
1881	439	5	1·14
1882	574	2	·35
1883	670	6	·89
1884	1,037	13	1·25
1885	1,581	14	·88
1886	1,722	29	1·68
1887	1,770	17	·96
1888	2,004	29	1·44
1889	2,197	29	1·36
	12,644	152	= 1·20 °/₀

Last 1,500 Private Patients.

	1,520	13	= ·85 °/₀

Evidence of Mr. BAMBRIDGE, Senior Examiner of the Midland Railway.

Every applicant for employment, and every servant of the Company on promotion, is examined as to their eyesight. The apparatus used is that which I show. The tests employed are Dr. W. Thompson's tests, consisting of a series of skeins suspended over a bar, and numbered with numbers which have reference to the colour. Three test skeins are used as standard skeins, the first a blue-green, the second a rose colour, and the third an ordinary scarlet. A candidate is required to match with the first test skein the skeins on the suspended bar, which comprise greens, greys, drabs, pinks, slate colour, and other colours corresponding to Holmgren's colours. (The tests were practically carried out after the Holmgren method.) The tests are carried out by daylight, though gaslight tests are sometimes employed. Doubtful cases are re-tested by Holmgren's plan. This method of testing has been in force for eight or nine years; before that the Army test was employed. The witness believed that the method now employed was very perfect. Should a signalman fall ill he is always tested before he is allowed to rejoin his post with the ordinary signals. In reference to colour-blindness produced by disease, he never saw a man who passed once fail on a further examination. It is quite possible that a man may fail in the wool test who rightly reads signals. The gaslight test takes place in a covered corridor with green and red lights; but, in addition to this test of signalmen, the wool test must also be passed. The position of the skeins of wool on the bar is not altered, and in case of doubt as to collusion the Holmgren test is adopted. About 2¼ per cent. of the whole who are examined fail. Sometimes a man may be allowed a second chance of examination if it appears that he fails through ignorance, but he never found that practice enabled a really colour-blind person to pass in a second examination. A man is always examined for colour-blindness after an absence due to an accident in case any alteration in his colour-vision should have occurred. As before said, a man is tested at every stage of promotion, and every applicant has to come to Derby for this purpose. With the aid of assistants, but under the witness's personal supervision, between 1,500 and 2,000 candidates for employment are examined each year, and in all 2,500 if old hands are included. Candidates are also examined for form, as in the Army test. The method is by means of dots separated by intervals equal to their diameters.

A distant signal is often three-quarters of a mile away from the signal box, and the signalman has to see if the arm works in the day time, or if the proper light is shown at night. An engine driver must see a signal about half-a-mile off in order that he may stop his train if necessary. Witness never heard of a case of an engine driver reporting a fireman for want of colour perception. Cases have been heard of in which the colour of light has been mistaken, and in such cases the man would be at

once suspended until he were re-tested. After a candidate has been tested at the office, he is sent to a medical man, and it has occurred that he has rejected a man who has passed the test. In such a case the man is tested for colour at the office again, and if he again passes, which he always does, he is not rejected for colour defect. All testing is done under the immediate supervision of the witness. Should a candidate show a slowness in selecting colours to match the test skeins, he would be reported as hesitating, and though the defect in vision might be trifling, he would be considered as unsuitable for an engine driver or for a signal man. Of the two tests, the witness preferred the heap (Holmgren's) test as the better, but it took longer to carry out than the bar (Thompson's) test, the latter only occupying a couple of minutes for each candidate.

The witness examined Mr. Rix, who is colour-blind, for his colour perceptions, and said he should not have passed him. He gave the following table of statistics to the Committee:—

Statistics respecting Colour-Blind Persons.

Half-year ending	Number of Candidates Examined.	Number found to have Imperfect Colour Perception.	Percentage.
June, 1884	722	20	2·77
Dec., "	1,019	39	3·82
June, 1885	·551	17	3·08
Dec., "	922	37	4·01
June, 1886	557	8	1·43
Dec., "	521	12	2·30
June, 1887	642	10	1·55
Dec., "	520	12	2·30
June, 1888	625	2	0·32
Dec., "	726	13	1·79
June, 1889	637	6	0·94
Dec., "	1,035	19	1·83
Average per annum..	1,413	32·5	2·18

Evidence of Mr. T. H. Bickerton, of Liverpool.

I do not know that I have got much more to say than I have already said in my pamphlets, although a few new facts have come under my observation.

The main point I have had in writing these pamphlets has been to point out to the Board of Trade in particular, and to the public in general, the great dangers incurred by the employment of colour-blind men, and of defective-sighted men, in positions

where the correct interpretation of coloured lights is essential to the safe navigation of vessels.

I have shown, and I think conclusively, the great difficulty there has been in the past in getting the Board of Trade to recognise these dangers, and that when at last they did recognise the dangers, they instituted methods of testing for colour-blindness which are not efficient, this being shown by the facts that these said methods, while they in very many cases allow colour-blind men to pass, in some cases cause the rejection of men as colour-blind who have a perfect colour sense. I have also shown that while the methods of testing are inefficient for the purpose intended, viz., the detection of colour-blindness, the regulations dealing with these colour-blind men when so detected are thoroughly bad. Colour-blind officers are granted the higher certificate, which is simply endorsed "This officer has failed in colours;" and the fact that he is colour-blind is no bar to his continuing in a responsible position. In the case of men applying for a Second Mate's Certificate, it is true he does not now receive his certificate, but he is at liberty to continue his profession. So far as the Board of Trade regulations go, colour-blind pilots, colour-blind "look-outs," colour-blind A.B.'s, and colour-blind apprentices are quite competent to assist in the navigation of ships, and may remain sailors to the end of their days. I believe that no regulations, however elaborate, with the object of preventing collisions at sea, and of preventing loss of life at sea, can be successful so long as men who have not good distant sight, and men who are colour-blind, are tolerated in the Mercantile Marine. Again, improvement in the methods of testing alone will not remedy the evils nor do away with the great hardships entailed on colour-blind men. At the present time a compulsory colour-test is only applied to those men wishing to advance themselves, and thus it is only after years of labour that their defect is discovered. To remedy the evils and the hardships, it is essential that a colour test be employed at the very commencement, and those who are colour-blind should be stopped before they begin the sea life. At the present time there is no test, and of a total of 956 boys who were being brought up for the sea life on training ships, I found thirty-four who were colour-blind. These were boys who were going to be sailors, and every sailor has responsibilities with regard to "look-out" lights, and I have proof that the large majority of these boys went to sea. I am told that the captains of reformatory training ships are compelled to accept boys even though they know them to be colour-blind.

The Chairman: Your first point is, that all these boys should be prevented from going to sea?

The Witness: Yes. The Board of Trade cannot settle this question by improving their tests unless they at the same time prevent colour-blind boys entering the Service. It seems to me the action of the Board of Trade all through has been inexplicable

At first they would not believe in the existence of colour-blindness; then when the dangers of colour-blindness could not be denied, they said the number of colour-blind cases were very small; and now they say the number of cases are so numerous that it would cause great hardship to rid the Service of them all. At the present moment no care whatever is taken to prevent colour-blind boys from being brought up to the sea life. Some three or four years ago I examined the boys of the training ships *Conway*, *Akbar*, *Clarence*, *Indefatigable*, and *Clio*, the first four ships being in the River Mersey, the latter in the Menai Straits.

On the *Conway*, out of 154 boys 2 were colour-blind. One, aged 14, had been on board two years; the other, aged 13½, had been there eighteen months. Both were fond of the sea; both were unaware of their defect; and both, on their friends being informed of the matter, were removed from the ship. On the *Akbar* there were 4 colour-blind out of 148 boys; on the *Indefatigable*, 12 out of 238; on the *Clarence*, 7 out of 158; and on the *Clio*, 9 were colour-blind out of 258 on board.

On these five vessels, therefore, there were at the time of my examination a total of thirty-four colour-blind boys being specially trained to a profession which they were physically and morally unfitted to enter. In addition to these, of 200 boys in the Seamen's Orphanage eight were colour-blind, and it is purely a matter of chance whether the boys have gone to sea or not.

Question.—Do you know if there is any examination in the case of the boys on the *Britannia*?

The Witness: I do not, but I should think there is. There is a careful examination as to form-vision, and they reject all boys who have not perfect vision of both eyes. Since the two colour-blind boys were discovered on the *Conway*, the Committee of that vessel have, I understand, insisted that every boy joining the ship shall bring a certificate stating that he is not colour-blind. I do not know if they are particular as to who gives the certificate.

Question.—Can you make any numerical statement as to persons on the seas whom you regard as unfit for their duties?

The Witness: I am aware of (*a*) eleven colour-blind men who were bound apprentices, and who at the time I was consulted had been at sea for periods varying from four and a half to eight years; (*b*) of four colour-blind able seamen whose years of service were respectively thirty-five years, twenty-one years, twelve years, fourth unknown; (*c*) of seven officers holding high and responsible positions, the length of whose services were respectively twenty-six years, eleven years, six years, ten years, twenty years, twenty years, thirty years, making a total of twenty-two colour-blind sailors. (In addition to these actual sailors, there are the thirty-four colour-blind boys, the majority of whom are now at sea, unless they are dead or left the service.) Some were obliged compulsorily to give up their

positions as officers owing to their being discharged by the owners. Whether they have gone to sea in the employ of less particular companies, I cannot say. I can only state positively that four of the twenty-two have not gone to sea. One of these four is the case of Captain John Smith, whose case has been brought prominently before the notice of Sir Baden Powell, who wrote to Sir G. G. Stokes about the poor fellow. The letter written by Capt. Smith, and published in the *Shipping and Mercantile Gazette and Lloyd's List*, dated 13th August, 1889, explains itself:—"On the 19th of June you were good enough to insert in your valuable paper a letter written by me on colour-blindness, and I am pleased to find that my letter and your article commenting on same has attracted considerable interest, notably by the Board of Trade. My object in again troubling you is to impress upon the Board of Trade the necessity for a more perfect means of testing sight. I have lost my position as chief officer in the employ of one of the best and most influential firms in this port, in whose service I had been for a period of six and a half years, and with a near prospect of command, through not being able to conform to owners' rule and produce a colour test certificate from their examiner, who, on the contrary, styled me colour-blind. I, however, doubted the accuracy of the report, and presented myself to an oculist, but found, alas! the Company's examiner's report too true. Now, I call this a very painful case, after being thrice passed by the Board of Trade for Second, First, and Master's Certificates. If the Board of Trade examination on any of these occasions had been true, I would have directed my energies towards another way other than the sea to obtain my livelihood. I may say that the defect in my vision has been, in the oculist's opinion, there from birth. I am now, morally and conscientiously, incapable of performing the duties of an officer on board ship at sea, though my Certificate bears no endorsement of any kind by the Board of Trade. Many owners I know do not require their officers to pass the colour-blind test, being satisfied with the Board of Trade Certificate. But I should think my case ought to be a warning to shipowners not to place reliance on the present Board of Trade test. My colour-blindness has destroyed my means of livelihood, and I fearlessly say that the Government test of sight is to blame for this. I am informed that I cannot claim compensation from the Board of Trade, because they have not interfered with my Certificate; but suppose I follow my avocation and get into collision through my defect, what then? and who would be to blame? I am a young man of thirty-three, and I have a wife and family depending upon me, and my position at present is very distressing. The best part of my life (Capt. Smith has been at sea for twenty years) has been passed in useless toil. My energies and prospects for the future have been unrewarded and blighted through no fault of my own, but through the lax and imperfect way in which I was examined and passed in sight by

the test that was adopted by the Board of Trade throughout the whole of my examinations."

When I first saw this gentleman on May 11th, 1889, a more hearty man than he appeared could not be. He had been getting £9 a month, and a bonus of £1 from his Company. The Company, on dismissing him from his ship, behaved very kindly, giving him shore employment at about £5 a month. But the loss of his situation, the having to give up the sea, and the destruction of his hopes so preyed upon his mind and body, that in May last he became the victim of acute phthisis, and died.

Up to the day of his dismissal he had not had a day's illness, nor had he had occasion to consult a medical man. The Board of Trade were well aware of the case, for on June 19th, after his letter had appeared in the press, he was sent for by the Liverpool Board of Trade, and asked if he was the writer of the letter, and his object in writing it; and, when he said it was in order to get employment, he was told to the effect that, as the Board of Trade had not interfered with his certificate, he had no claim upon them, and that if shipowners chose to make laws for themselves, it had nothing to do with them, and did not prevent him going again to sea, as he could go to other companies. It must not be thought this is an isolated case. It is now no uncommon thing in Liverpool to hear of officers being dismissed for colour-blindness who have held, in some cases for years, lucrative and responsible appointments on board ship. Everyone will admit the justice of these dismissals, for upon the correct colour-vision of the officer on watch depends the safety of the ship, and, in many cases, the lives of hundreds of helpless passengers, and property to the extent of hundreds of thousands of pounds, but everyone will at the same time admit the hardship—nay more, the injustice—done these men by the use of bad Government tests and regulations. This brings me to another point, that many of the shipowners of Liverpool will not take a Board of Trade certificate now. Up to the time I wrote my second pamphlet the Liverpool shipowners believed that the Board of Trade certificate was a positive proof that an officer was not colour-blind. Now many of them refuse to take it. They do not test the men themselves. Many of them send their officers to medical men or to opticians, or ask them to again go to the Board of Trade, and I may here mention that sailors have a considerable objection to being tested by opticians; and I have been told of a case where a sailor, on being rejected by a surgeon attached to an Atlantic liner, remarked, he "didn't see why he should not go to sea, because a common ship's doctor said he was colour-blind." These men have the Board of Trade certificates already, but since I pointed out the defects in the Board of Trade tests, many of the owners of the large Atlantic passenger steamers insist on a re-examination of their officers' colour-sight and form-sight.

I think there should be an efficient examination in the first instance. No improvement in the mode of testing can be satis-

factory unless it is applied at the threshold of the sailor's career, and not, as at present, when about to obtain the reward of his years of labour. Before an apprentice or man be allowed to put his foot on board ship as a sailor, he should be compelled to produce to the Captain or Shipping Clerk a certificate of good colour-sight. The matter entails no difficulty. At the present time a sailor is obliged to keep by him his various certificates of discharge, it would be no hardship for him to keep a colour certificate also.

The CHAIRMAN: How can we give a numerical value to your observations? You know of several cases of officers who are colour-blind, and are sailing the seas, to what extent can you give percentages?

The WITNESS: It is difficult to do this, but we may presume that the percentage of congenital colour-blindness among sailors is the same as that among any other community of males, and by taking the average of the percentages given by three reliable authorities:—

Holmgren	examined	32,165 men—	1,019	colour-blind,	3·168	per cent
Joy Jeffries	"	10,387 "	431	"	4·149	"
London Com.	"	14,846 "	617	"	4·156	"

this is found to be 3·824. By the census of 1881, the number of sailors in the Mercantile Marine Service in England was 95,093; in Scotland, 14,143, and in Ireland, 10,886; making a total of 120,122; and this does not include such men as pilots, canal or lighter men. Calculating 3·824 per cent. of this number to be colour-blind, we have a total of 4,593 men holding at the present time positions in which the correct interpretation of coloured lights is essential.

I am not making allowance for those rejected. But I might call attention to the great variations in the Board of Trade percentages of rejections, which render their report unreliable. In the official report, published in February, 1885, it is stated 123 men were colour-blind out of 21,720 examined, this giving the percentage of ·586, and a careful study of this report will show that thirty-one out of eighty-five colour-blind men eventually were granted unendorsed licenses. But the public attention called to this question has raised the percentage, for we are told, in the report of 1888, that between the months of January and May, no fewer than 320 sailors were examined by the Superintendent of the Mercantile Marine, at Tilbury Docks, and among them sixteen, or five per cent. were found unable to discriminate red and green in the degree requisite for safe navigation. This percentage one may positively state is as ridiculously high as the former quoted is ridiculously low. Something therefore must be wrong, either with the tests themselves, or with the way in which they are applied.

All who have consulted me have done so on account of their colour-blindness. A very considerable number of these came to me because they did not believe they were colour-blind. The defect

had been found out accidently, or owing to their being compelled by their employers to undergo a re-examination, as to their colour-vision by the Board of Trade or by opticians. I examined them for colour-blindness by all ordinary tests.

I use a good many, but Holmgren's is the one that I trust fully. I have not kept records of those who were not colour-blind; but they were very few, for there could be no reason for a man who was not colour-blind coming to see me. I can, however, remember two cases, and one of them whose case is fully quoted in Pamphlet No. 2, page 7, shows clearly that by the present Board of Trade testing a man who is not colour-blind may be, by their tests, rejected as colour-blind.

The CHAIRMAN: Taking any one company, can you form any idea as to how many officers in their employ are colour-blind?

The WITNESS: No, because I do not examine for any company; but Dr. Hodgson, of Bootle, who examines for the Cunard Company, told me he rejected five out of 120 officers in the employ of the Cunard Company for diseases of the eyes. This company, long before the Board of Trade took up this matter of the sailors' eye-sight, recognised the grave responsibility resting with them in the selection of men (look-outs and officers) for a duty which they considered of paramount importance. For this they deserve every credit, and it is no doubt one reason of the freedom this company has had from disaster (*vide* Pamphlet 3, page 12).

The CHAIRMAN: Have you details of the diseases of the rejected men?

The WITNESS: No, but the same doctor quoted the case of an officer who could not tell the colour of his ship's funnels, and did not know that the fluid issuing from his nose on one occasion was blood, until told by the bystanders.

The CHAIRMAN: The point you want to bring before the Committee is that a test for colour-vision should be instituted at the commencement?

The WITNESS: Yes, at the very commencement, and those who have not perfect colour sight, and also good distant sight, should not be allowed legally to enter the service at all.

The CHAIRMAN: You wish further to point out that the methods of testing by the Board of Trade are wholly insufficient?

The WITNESS: Yes.

The CHAIRMAN: And that although you are not able to make a numerical statement, you are convinced there are many persons now in the Mercantile Service who are colour-blind?

The WITNESS: Yes.

The CHAIRMAN: There are a number of training institutions for the poor where destitute boys are sent, I believe?

The WITNESS: Yes; the *Indefatigable*, *Akbar*, *Clio*, *Clarence*. To the three latter vessels boys brought before magistrates for vagrancy are sent without any reference being made as to their fitness for the sea life. Everybody to be employed as

sailors should be examined as to their colour-vision. I do not include firemen and stokers. At the present time individual ship-owners have the men in their employ tested; but this is of little avail unless all men in every company are tested, for it takes two ships to make a collision.

The Chairman: Have you ever thought whether it is feasible by altering the coloured signals, say by substituting a flashing light as in the army, the difficulty might be got over?

The Witness: Yes, I have thought of it, but I believe it to be impracticable. The shipping men themselves say so. In this question of coloured light there is one eminent gentleman who has, in my opinion, done much harm. Admiral Colomb has been a great power in preventing this subject of colour-blindness receiving the attention it deserves. I have in Pamphlet 2, page 11, given my reasons for believing the means recommended by "Select Committees for the Prevention of Loss of Life at Sea" are and must remain futile so long as the very essential of safety, namely, perfect eyesight on the part of officers and men, is ignored. Admiral Colomb thinks differently, but, as I believe, wrongly, and I would have no hesitation in taking the popular vote on the point between us. In the course of an able paper delivered by him on the subject of the Washington Maritime Conference, at the Society of Arts, and reported in *The Times* of March 28th, 1890, he made the following remark:—

"As to the qualifications for officers and seamen, the Conference (Washington) dealt wholly with the question of colour-blindness on account of its danger with reference to the red and green side lights. He never knew himself a case of collision where colour-blindness was in question. The statements were generally perfectly clear that wrong helm was given deliberately in the face of the colour seen, and as no authoritative teaching had existed to show that it mattered what colour was seen so long as danger was denoted, he had never been able to lay stress on the colour-blind question."

Mr. Baden Powell, R.N.R., who followed in debate declared "that in all cases of collision at sea there was no default of the rule of the road at sea, but they generally arose from negligence. The rule of the road at sea was perfectly well understood by intelligent men, and it was the 'lubbers' and the careless who did not act according to it."

Admiral de Horsey considered "collisions at sea were caused principally by three faults—a bad look-out, ignorance of the rules, and neglect of the rules."

In his reply Admiral Colomb "expressed his opinion that collisions at night occurred through the helm being ported to the green light, and starboarded to the red; and he could not agree that the collisions occurred wholly through negligence, for he thought that they largely occurred because our seamen were not taught what they should do, and the collisions occurred through ignorance."

Now I say that there are a number of well authenticated cases where disaster due to colour-blindness and to defective sight actually occurred, or was narrowly averted; and it is surprising Admiral Colomb does not know of them. I would also ask whether Admiral Colomb knows of a single case out of the thousands that have occurred where, after collision, the colour sight of the officers and men was tested by an expert. Would it not be as well if Admiral Colomb were to eliminate this cause before denouncing it? One might have thought that, as in most shipping enquiries, the evidence as to the colour of the lights, and as to the distance at which they were first seen, is bewildering in its contrariness, the first step towards a solution would be to examine on the spot the far sight and colour sight of the witness; but those who adjudicate at these enquiries think differently, and take it for granted that the witnesses coming before them have perfect far sight and perfect colour sight. It is my opinion that if the eyesight of sailors on colliding vessels were tested in Court, we should find that the cause was in many cases neither ignorance nor negligence, nor due to "lubbers," but that it would be found in the colour-blindness or defective sight of the officers and men on watch.

Capt. Abney thought Admiral Colomb must have realised the fact that there are mistakes as to the colours, and was a man very much open to conviction, and ready to adopt improvements. He could not think he was antagonistic to anything in the way of advance.

The Witness: In opposition to the opinion of these eminent gentlemen, I will quote the opinion, in which I fully agree, expressed by a gentleman who wrote to the *Liverpool Daily Post* in the following terms:—

"Is it reasonable to believe that steady married seamen with families depending on them, and who have had years of experience, suddenly lose all judgment and common sense, and steer their vessels on clear nights, sometimes in broad daylight, so as to deliberately ram each other, thereby losing their lives and ships, and the lives of the passengers? Surely not. In none of the other professions or callings can we find anything approaching a parallel case; therefore, in some cases their eyesight must be defective."

If Admiral Colomb would only take the trouble to examine personally a colour-blind officer, I feel sure that this subject would have in him a distinguished convert and an able and powerful advocate.

The Chairman: Have you any special evidence to give as to accidents?

The Witness: Yes, in Pamphlets 1, 2, and 3 I have related many cases of accidents due both to colour-blindness and to defective sight on the part of those in charge of the vessels. The Liverpool Board of Trade use the ordinary Board of Trade tests.

The CHAIRMAN: What authority has the Liverpool Board of Trade? Is it simply limited to Liverpool?

The WITNESS: Yes, the powers of the Central Board of Trade are given over to the Liverpool Board for Liverpool.

Question.—Will you give us your practical experience with regard to different methods of testing?

The WITNESS: I have very little hesitation in saying that all tests requiring a man to name colours are defective. Practically, that brings them down to Holmgren's, which is the simplest, and, for ignorant men, the best one. I consider that test perfectly trustworthy, and it has one great advantage—it can be applied irrespective of nationality.

The CHAIRMAN: As a matter of practical experience, about what time do you find it takes in using Holmgren's test?

The WITNESS: I calculate about 40 boys an hour, or 100 girls in the same time. The time taken depends a great deal upon the social standing of the children. At Eton or Harrow it would be very different to that at reformatory schools, where perhaps only thirty boys could be examined in an hour, as they are so ignorant that a test has to be explained to them over and over again in order that ignorance may not be mistaken for colour-blindness. I am acquainted with Professor Grossman's test, and I think it a test for experimental purposes, but not for practical use. Captain Smith, of whom I have already spoken, passed it without difficulty. He was examined by the Board of Trade card test and lanterns. He was then asked by Sir George Powell to see Dr. Grossman, and Captain Smith told me he named the letters rightly. I tested him on two or three occasions with Dr. Grossman's test, and he never made a mistake.

Question.—Did Dr. Grossman supply the test you used?

The WITNESS: I got it from the optician from whom he said it might be obtained. In many cases it would be difficult to reject with this test, although feeling sure the candidate was defective. An educated colour-blind man would get through. There is also the disadvantage that it takes considerable time to apply.

The CHAIRMAN: What was the nature of Captain Smith's colour-blindness?

The WITNESS: I do not for the moment remember. I do not find the great distinction which is laid down between the different kinds of colour-blindness. The one class appears to run into the other.

I think different classes do exist, and in great number.

I have not examined cases with regard to the shortening or non-shortening of the spectrum.

The CHAIRMAN: You have no suggestion to make with regard to tests?

The WITNESS: None, except that I pin my faith to Holmgren's test applied by an expert examiner, and carried out according to Holmgren's instructions.

In connection with this test I should like to hand in a letter which I received lately from Mr. Clement E. Stretton, C.E., of the Associated Society of Locomotive Engineers and Firemen:—

"40, Saxe-Coburg Street,
"Leicester,
"*December* 21*st*, 1889.

"DEAR SIR,—I am always glad to read a letter on the eyesight question, as I trust it will all lead to something being done to avoid that which may soon turn to a strike against 'dots and wool.' In order to save the men their situations the Railway Societies are having the men taught in wool shops with first-rate results.

'The present tests are useless for railway men, and very unfairly applied when required to get rid of the men.

"The *Mechanical World* and *Invention* of to-day each have important information upon the subject.

"I would strongly advise you to apply to Mr. Harford, Railway Servants' Society, 55, Colebrooke Row, London, N., for the practical side of the question.

"Yours truly,
"CLEMENT E. STRETTON."

The complaint which Mr. Stretton makes as to the unfair way in which railway men are treated is no doubt grounded on just cause, but he is in error when he attacks, as he has on many occasions done, Holmgren's test, which has with reason been accepted as a reliable one by those more competent to judge. The "dot and wool" test of the Railway Companies is not the wool test of Holmgren, and the fact, as stated by Mr. Stretton, viz., that the railway men can be educated to pass the test is, if any were required, positive proof that the test applied to them is not Holmgren's. It would, however, appear that there is cause to believe that the men are badly treated, as the following letter will show; and until Holmgren's test becomes the official test, and is applied by those who understand its use, and who are in an independent position, the friction which is at present felt is likely to continue:—

"Amalgamated Society of Railway Servants,
"Head Offices, 386, City Road, London,
"*March*, 1887.

"DEAR SIR,—At the last Meeting of the Executive Committee the testing of the eyesight of drivers, firemen, guards, signalmen, and other servants of the various Railway Companies was considered, and from the facts submitted it was felt that the usual tests were often most unfairly applied, more especially in the case of the older servants, and that, in consequence, men were being reduced or removed from the service under the plea of defective sight. From this there being no appeal, the Executive Committee considered that the tests were being used so as to

give a pretext for getting rid of men who have grown grey in the service, and whose lengthened experience and faithful servitude should entitle them to some consideration.

"In order, then, that men so tested may have the opinion of their fellow members as to whether their sight is defective, I am instructed to enclose you a card used for testing sight with which members may test each other, and in the event of the unfair tests being used by the Companies' officials, a reliable protest can then be made, backed up by the verdict of the branch, which would, of course, submit any member said to have defective sight to the usual tests in order to satisfy itself, before expressing an opinion.

"It may also be found advantageous to frequently use it when no such cases require to be decided, so that members may be familiar with its use, and so be prepared to undergo the examination whenever called upon.

"Printed instructions for using the card will be found on its back.

"I am, dear Sir,
"Yours faithfully.
EDWARD HARFORD,
"*General Secretary.*

"To the Branch Secretary."

The CHAIRMAN: Have you any knowledge as to what the test was that was issued with this circular?

The WITNESS: Yes, the ordinary railway test card, having printed on it the small square dots and spaces, and the colours red, green, yellow, and blue; and this test is an absolutely useless one.

The CHAIRMAN: You think it impossible to get a colour-blind through Holmgren's test?

The WITNESS: A congenital colour-blind. Yes, impossible.

The CHAIRMAN: Have you any special evidence to give as to accidents?

The WITNESS: I have given cases in my pamphlets. The first case is to be found in the Annual Report of the Supervising Inspector-General of Steam-boats, to the Secretary of the Treasury, dated Washington, 1880, and reads as follows:—

"On the the night of the 5th July, 1875, there was a collision near Norfolk, Virginia, between the steam-tug *Lumberman*, and the steam-ship *Isaac Bell*, the former vessel bound to, and the latter from, Norfolk. The accident occurred about 9 p.m. on an ordinary clear night under circumstances which, until recently, seemed more or less mysterious. The master of the steamer and all his officers made oath that at the time signals were made to the tug, the latter was from one to two points on the steamer's starboard bow, and consequently the steamer's green light only was visible to the approaching vessel. Yet the master of the tug, whose statement was unsupported by any other testimony, asserted that the steamer's red light was exhibited and signalled

accordingly. The discrepancy in the statement was so great that many persons uncharitably charged the master of the tug with being intoxicated, although no evidence was offered in support of the charge. By this accident ten persons lost their lives. Upon a visual examination of this officer under the rules during the past summer, and during which time there had been no question as to sight by the Sergeant of the Marine Hospital at Norfolk, he was found to be colour-blind, two examinations having been accorded him, with an interval of ten days between them."

A second case is mentioned in the *Shipping and Mercantile Gazette and Lloyd's List*, dated 29th June, 1881:—

"The pilot of the *City of Austin*, which was lost in the harbour of Fernandia, Florida, last April, is proved to be colour-blind. In this way it would appear he mistook the buoys, and his mistake cost the owners 200,000 dollars (£40,000). An examination showed that at a distance of more than six feet he could not distinguish one colour from another. The physicians attribute the defect to an excessive use of tobacco. The services of the marine surgeons were tendered to the local authorities without fee two years ago, but were declined."

A third case is recounted in a letter from Messrs. Macintyre & Co., Liverpool, shipowners:—

"Our ship *Carbet Castle* collided in the South Channel, bound from Dundee to Cardiff, in 1879, with the *T. H. Ramien*, due, as far as we can make out, to the colour-blindness, or short-sightedness of the chief officer."

The following account is written by Captain Coburn, who was for many years in the employ of Messrs. Leach, Harrison and Forwood, of Liverpool, and is to be found in the *Mercantile Marine Reporter*, vol. xiv, No. 162:—

"The steamer *Neera* was on a voyage from Liverpool to Alexandria. One night, shortly after passing Gibraltar, at about 10.30 p.m., I went on the bridge, which was then in charge of the third officer, a man of about 45 years of age, and who up to that time I had supposed to be a trustworthy officer, and competent in every way. I walked up and down the bridge until about 11 p.m., when the third officer almost simultaneously saw a light about two points on the starboard bow. I at once saw it was a green light, and knew that no action was called for. To my surprise, the third officer called out to the man at the wheel 'port,' which he was about to do, when I countermanded the order, and told him to steady his helm, which he did, and we passed the other steamer safely about half a mile apart. I at once asked the third officer why he had ported his helm to a green light on the starboard bow, but he insisted it was a red light which he had first seen. I tried him repeatedly after this, and although he sometimes gave a correct description of the colour of the light, he was as often incorrect, and it was evidently all guess work. On my return, I applied to have him removed from the ship, as

he was, in my opinion, quite unfit to have charge of the deck at night, and this application was granted. After this occurrence I always, when taking a strange officer to sea, remained on the bridge with him at night until I had tested his ability to distinguish colours. I cannot imagine anything more dangerous, or more likely to lead to fatal accidents than a colour-blind man on a steamer's bridge."

A similar experience is thus related by Captain Heasley, of Liverpool:—

"After passing through the Straits of Gibraltar, the second officer, who had charge of the deck, gave the order to 'port,' much to my astonishment, for the lights to be seen about a point on the starboard bow were a masthead and green light, but he maintained that it was a masthead and red, and not until both ships were nearly abreast, would he acknowledge his mistake. I may add that during the rest of the voyage I never saw him making the same mistake. As a practical seaman, I consider a great many accidents arise from colour-blindness"

In the collision which occurred in February, 1889, between the steamship *Nereid* and the sailing-vessel *Killochan*, the vessels had had each other in sight for at least two miles, and it was a perfectly clear night. *The Times*, in commenting on this disaster, remarks, February 5th, 1889, that "all inquiries respecting the cause of disaster lead to the same conclusion, that it was due to one of those astounding errors of judgment on the part of one or other of the navigators, which seemed to deprive all attempts at reasonable excuse. Each blames the other."

As we know that there are many colour-blind men holding officer's certificates, it will not be surprising if it were found that the officer in charge of the steamship *Nereid* was colour-blind. The explanation of the accident would be similar to that first quoted, namely, that he mistook the green light for a red one, and ported in order to go, as he erroneously would think, astern of the *Killochan*.

So long as colour-blind men are tolerated in the Mercantile Service, these accidents will occur.

Question.—But could not many of the people on board have seen how these accidents occurred?

Yes, and the evidence in these cases is always conflicting. Everybody will remember the loss of the *Oregon*. It was said to have been run into by a coal boat. The evidence was contradictory, the light seen being described as white, red, and green.

But the idea of examining the men's colour-sight was never thought of. In the following case the steamer *Toronto* on the night of January 18th, 1888, ran down the Norwegian barque *Freidis* in the Irish Channel, on which occasion thirteen lives were lost. The evidence given at the Board of Trade enquiry as to the lights seen may be briefly summed up as follows:—The captain, the mate, and the quartermaster saw first a red light and then a green one. The look-out man saw no red light, only

the green light. Asked if he was colour-blind, he replied that he was not, and that he had never made a mistake in reporting the colour of a light; and, in answer to the question as to what in his opinion was the cause of the collision, he had no hesitation in stating that it was owing to his own captain porting his helm. In a letter published and commented upon in the leading Liverpool shipping paper, the *Journal of Commerce*, referring to this case, it is remarked that "the negative evidence of the look-out man that he did not see the red light cannot weigh against the positive evidence of the captain, two officers, and the quartermaster that they did see it, and it has yet to be ascertained why it was not seen by him." But the Court chose to take the look-out man's statement as against that of the officers. The officers of a ship being considered the responsible men navigating a ship are therefore tested for colour-blindness by the Board of Trade, but the Board of Trade do not admit that the look-out men are responsible. They argue in this way:—

It is not for the look-out men to say what the colour of lights are they see. They merely have to report that there is a light, and it is left to the officers to say what that light is; but when collision cases come into Court the Judges invariably ask the "look-out" as to the colour of the light seen, and as often as not take the word of the irresponsible "look-out" against that of the responsible officer, who is supposed to do the best for his Company, and who is also on his trial.

Mr. Bickerton subsequently communicated the following results of an examination for colour-blindness that he held:—

"I examined again on Monday and Tuesday last the boys of the Seamen's Orphanage in order to obtain some cases for a lecture. The results of the two days' examination were most curious—

First day—91 boys examined	..	1 colour-blind.
Second day—44 „ „	..	5 typical blind.
	or 6 colour-blind in 135.	

Total number of boys 225; but I had no time to examine the remainder. On examining the same institution five years ago there were 8 colour-blind out of 200. All the children are, as the name of the Institution implies, the sons of sailors. That fact, in chief, is of interest when the hereditary quality is taken into consideration."

Evidence of Mr. E. Nettleship.

Prof. Foster: You have kindly consented to put the information you possess concerning Colour-vision at the disposal of the Committee, and we must leave it to you to decide the points upon which you will give evidence; but there is one class of cases we should particularly like to know something about, namely, those of scotoma from diseases of the optic nerve?

The WITNESS: It generally affects both eyes, and causes a *lowering*, but seldom complete loss, of the functions of the central part of the visual field. Except in very severe cases perception of black and white remains, but there is, I believe, always a disproportionate lowering of perception of colours over that area. I believe the usual form it takes is blindness to the complementary colours—red and green.

For detecting the presence, and roughly estimating the size and density, of the defective area (scotoma), it is enough to use a small piece of coloured paper on the end of a stick or a pen; the coloured piece should vary in diameter from 5 mm. or less, up to 25 mm. or more, according to the severity of the case; the more the sensibility to red, *e.g.*, is lowered the larger must the retinal image be, *i.e.*, the greater the number of units excited, in order that the sensation of red may be produced; also the greater the defect the brighter must the colour be. For accurately mapping the scotoma of course the perimeter must be used. As the loss of colour perception on the greater part of the defective island, and often over the whole of it, is only partial, the size of the scotoma and its exact outline, like the size and exact outline of the normal field for any colour, vary according to the size and quality of the colour used, and also to some extent with practice and attention on the patient's part.

I usually take red first, because any defect in that is most easily apparent; it is not so easy to get a pure green, and many people are uncertain between blue and green, or do not know the names. In very slight cases, however, we sometimes use a pale green in preference. The green I use is as pure a light green as I can get. "Emerald green" conveys to me the idea of a bluish-green, but perhaps erroneously. Light-green baize would be the colour, I should think.

Question—Could you describe the green you mean in wave-length?—I have no knowledge of colour expressed in terms of wave-length.

The detection of the scotoma depends in a certain degree on the luminosity of the test-colour employed; *cæt. par.*, the lower the saturation of the coloured spot, and the smaller the diameter of the coloured spot, the more easily is the defect perceived (*see* answer to a previous question).

I do not test with mixed colours: for instance, purple I found unsatisfactory, except in slight cases of tobacco amblyopia, where you must either take an extremely small spot of pure colour or a larger spot of carefully mixed colour. Such patients will sometimes say mauve is red. Something depends on the patient's training and intelligence. I had a case of central scotoma from tobacco smoking in a man who had been accustomed to deal in artist's pigments; he recognised every colour, pure and mixed, in spots of various sizes, till I tried a dark sort of mauve, which appeared to him blue or bluish in the centre of the field. He said that the only commercial colour with which he had had any difficulty was

"smalt." [The witness here handed in a number of charts illustrative of cases of tobacco amblyopia, and atrophy of the optic nerves, and explained with reference to them.]

Question—They do not lose the sense of form, only the colour?—The test is used only for its colour. The form of the spot is not spoken of, and is of no importance.

Question—Is not the defect of Colour-vision generally accompanied by defect of Form-vision?—Yes, always. I have never seen a case in which the loss has been entirely a colour loss. The form loss (loss of acuteness of vision at the centre of the field) is always recorded first. Speaking broadly, the loss of form-sense, as it is commonly tested, *i.e.*, by black letters on a white ground, is about proportionate to the loss of colour-sense at the centre of the field. One commonly records the form-sense, however, only at the exact centre of the field, whilst in the cases of central (or approximately central) scotoma, one tests the loss of colour-perception over an area extending several degrees from the centre in every direction. If the scotoma area be represented as a cloud, we shall have to say that in different cases the total area of the cloud varies, as well as its average density, and that its nucleus or densest spot, though always very near to the exact centre of the field, seldom coincides precisely with that point, being usually 2 or 3 degrees to its outer (temporal) side, sometimes inclined upwards, sometimes downwards. The cloud usually forms an oval, extending further into the outer than the inner part of the field, and frequently including the blind spot. If the cloud be very large it may be co-extensive with the field for green or for red, and then those colours will not be recognised anywhere; but in the ordinary tobacco cases the cloud is smaller than the red field, if not smaller than the green field, so that a red-perceiving zone is left of greater or less width and perfection. (Several of the charts illustrate these various points.) In the cases to which I have referred the patient has come for advice on account of defect of Form-vision, and has seldom said anything about Colour-vision. One of the first complaints made is often of a mistiness that prevents the patient from recognising the features of a person at a distance (the scotoma when small covering the person's face at a distance), and at the same time of difficulty in reading, which is not removed by spectacles. Occasionally they will say that people's faces look unnaturally pale. I have known two cases in which sportsmen found out the defect whilst very slight by their bad shooting: they could see the birds rise (eccentric vision) but just when they aimed the bird "was lost."

Question—When you have had a man who could not see red, and have shown him a red object, say a bit of sealing wax, what does it look like to him?—In accordance with what has been already said this will depend largely upon (1) the size and (2) the brightness (saturation) of the test object. If it be three or four inches long, and an inch or so wide, he will (unless very bad) usually recognise the colour, either because some part of its

retinal image falls on the undamaged part of the red-perceiving field, or because, though the image falls on none but damaged percipient elements, it occupies so many of them that a correct sensation is the result. But if the red test be of from 5 to 20 mm. diameter, the patient will call it variously "no colour," or "brown," or "black," or later on, perhaps, "white;" finally, after having recognised it correctly in the eccentric parts of the field, he may continue to recognise it correctly, though as a "paler" a "duller" red, even at the centre. If a light-green spot be used as the test, it will commonly be called "white," sometimes "grey," on the defective area. I think they never call pure red yellow; certainly not often. They more often call it brown.

Question—With reference to tobacco amblyopia, is there any particular kind of tobacco you have found to cause it more than others?—All strong tobaccos, especially "shag," cavendish, and strong cigars.

Question—Are cases sometimes caused by alcohol, or by tobacco only?—There is a great difference of opinion as to whether alcohol alone can produce this form of amblyopia, or any form at all commonly. Some think it often causes central amblyopia, though I have never seen a case where alcohol alone had done so. There is abundant evidence that tobacco alone can cause it in cases of teetotalers. We also know tobacco cases occur in women smokers. A large number of carefully recorded facts by various observers, bearing on the influence of alcohol and many other points in relation to this so-called "toxic" amblyopia will be found in the "Transactions of the Ophthalmological Society of the United Kingdom," vol. vii, p. 36 (1887).

Question—In cases of persons who are heavy smokers of strong tobacco who get amblyopia, is it not usually some mental depression such as would be caused by the loss of a wife or child that causes the tobacco to take effect?—Yes; I have for many years insisted strongly on the frequency with which the onset of failure of sight in smokers has been preceded by something which, directly or indirectly, has caused a lowering of general vigour. It is comparatively seldom that when tobacco amblyopia comes on the subject is in his usual vigour and health.

Question—Do you think these cases of scotoma in railway servants and others are not common enough to be worth consideration?—They are not at all so rare as to be unimportant from the point of view of signal reading, but the safeguard is that they always suffer from defects of form-sense, and that causes them so much inconvenience that they take advice for it. Though they might now and then manage to carry on their signalling duties for a time, such an event would be rare.

Question—Do you find that people engaged in the open air suffer less in this respect than those employed indoors, such as clerks?—I do not know that it affects any class particularly, apart from depressing or exhausting causes.

Question—You think it would be advisable after a railway accident to test the driver and guard for central amblyopia?—Yes; I think it would be well to do so some little time after the accident, since shock is one of the causes of the lowered vigour which so often precedes this failure of sight. I had the following case in point:—A railway servant jumped off the foot-board of a train moving at about 10 miles an hour. He was badly shaken, and his general symptoms were for a time suspected to indicate grave degeneration of the brain and spinal cord. His sight also failed, and this was also thought to point in the same direction, until it was found, on careful examination, that he had the scotoma of tobacco amblyopia, and that he smoked. His sight returned perfectly when he left off smoking; he also gradually recovered from the symptoms of shock.

Question—Your opinion is that, contrary to the ordinary so-called colour-blind persons, these people with central scotoma have a sufficient defect of form-sense to warn them?—It is always great enough to be a safeguard. It is the same with other diseases of the optic nerve, but the clinical features of cases of atrophy of the optic nerve, from whatever cause, are, generally speaking, less uniform than those of the axial neuritis that occurs commonly from tobacco smoking, and perhaps occasionally from other toxic influences, and as a substantive disease. The *axial neuritis group* presents tolerably uniform symptoms, because only certain bundles of fibres of the optic nerve are diseased, viz., those which supply the central area of the retina, the disease very seldom spreading to the other bundles. The symptoms in other forms of optic nerve disease are less constant, because the malady does not show any such constant selective affinity for certain strands of fibres, but may affect some or all, and with various degrees of severity and of permanence, according to the seat and nature of the originating cause. In one very important group of cases, the group known generally as "progressive atrophy" of the optic nerve, it is the rule to find that the field of vision in the earlier stages is curtailed at its circumference, either all round ("concentric contraction)," or more commonly by the loss of sector-shaped pieces. Together with such *total* loss of portions of the field there is usually a lowering of sensibility over the area that remains, so that "acuteness of vision" is damaged also; but sometimes the centre remains very good in spite of great loss of peripheral vision. This "progressive atrophy" is most commonly a part of a similar disease affecting the spinal cord (and sometimes the brain) in the form of tabes dorsalis or locomotor ataxy. Marked colour-blindness is the rule in progressive optic atrophy, but, according to my own rather rough clinical notes, the loss of colour perception does not stand in a perfectly uniform relation with loss of (central) acuteness or with loss of field;*

* I have not collected any observations on this point since publishing such as I then had, in 1883, in vol. iii of the "Trans. of Ophth. Society," p. 256.

nevertheless, in by far the majority of cases of this sort, as in central amblyopia from tobacco, &c., acuteness of vision is so much lowered as to be for the patient the most important symptom. I remember only one case in which the patient, a sailor, who had been accustomed to steer and to look out, discovered his inability to distinguish the colour of ships' lights, whilst his acuteness of sight still remained good enough for ordinary purposes about the ship. (Wm. B. "T.O.P.," iii, 33.) How the loss of colour-vision stands in relation to loss of light perception in cases of optic nerve disease, I cannot say. People suffering from disease of the optic nerves often come saying they want glasses for failing sight, and naturally think the defect can be remedied by glasses. Glasses do not help the matter, though they of course may remedy the form so far as the defect is due to the images not being properly focussed.

Question—It has been alleged on behalf of railway workers that the sense of colour-vision becomes impaired after long hours of work and want of rest?—I have no evidence on that subject; but I should not have thought it likely to be true.

Question—Have you had any experience of hysterical colour-blindness?—Yes, but I have not put together my facts about it. Contraction of field and lowering of acuteness of form-perception are nearly constant; but the state of colour-perception, according to my experience, varies greatly. The fields sometimes show spiral contractions, but not always. The spiral contraction is, as a rule, put down to exhaustion.

About congenital colour-blindness I have not much to say, except that it is common in men and very rare in women. I have one splendid case of a colour-blind woman—the only one I have ever seen of the kind. It is not common green-blindness; I think it must be blue-blindness. She never makes mistakes about green, and is always wrong about other colours. I use Holmgren's test.

Question—You have had some experience with the use of a lamp? Can you make any statement about that?—The results with the lamp vary very much. I cannot quote statistics; but I think it is true that as a rule people congenitally colour-blind will make fewer mistakes with such a test than those whose colour-defect has come on with disease of the optic nerve, *e.g.*, tobacco cases. But both classes are liable to make mistakes if taken off their guard by varying the colour of the glass, the size of the aperture, or the brightness of illumination. A further cause of variation lies in the interest or attention that the examinee shows; he may make mistakes at first, but learn to correct them if the tests are repeated. I should not myself, as at present advised, rely upon a lantern-test alone, either as a scientific test of colour defect, or as a trustworthy guide for the detection of those whose colour-defect is dangerous. The wool-test is much less open to these objections, because the number of tints and shades is so much larger, and possibly

also for some optical reasons. In my testing lantern I have a diaphragm with holes of different sizes, representing the regular railway lights at different distances, with red and green glass supplied by one of the Companies, and in addition a number of bits of smoked glass. I often, but not always, succeeded in getting the persons tested to confuse red and green, and also white smoked light with coloured lights. The persons tested should stand at a distance of 12 feet, and I vary the size of the hole so as to represent a railway signal at different distances. Some time ago my colleague, Mr. Lawford, examined an engine driver from the South Western Railway, who came to St. Thomas's Hospital because he had been rejected that morning on account of colour-blindness, although he had been two years previously tested and passed. His eyes were perfect but colour-blind. With Holmgren's first series he matched green and red; and with the next series he confused greens and greys. He was tried with the lamp, and then confused red and green in both large and small dots, making more mistakes with green than with red glass. Such a man, if he were very much on the watch, might go on for many years with safety. He said he never experienced any difficulty in telling the colours of the signals because the red "glistened." I had a somewhat similar case with a medical student who had been at sea. In trying him with one of Stilling's tests, consisting of coloured letters on a black ground, I found he could not see green on black. I asked him if he could tell lights at sea, and he said, "Yes, quite easily; there is the red light and the black light." I lately saw a man who had been a stoker on the Great Eastern Railway for a number of years and now wanted to pass as a driver; but on being tested was rejected as colour-blind. He did not believe himself to be so, and came to Moorfields Hospital for a certificate that his colour-vision was perfect. He proved, however, to be an ordinary red-green blind. On the other hand, we have had men appealing to us there because they had been rejected who really saw colours quite well.

Question—That probably occurred through using the naming tests?—Yes, I suppose so.

Question—Does night-blindness throw any light on the question of colour vision?—I do not think so. Patients who have night-blindness are certainly not colour-blind. In such cases I think all colours disappear equally, together with form, but I cannot speak with any authority on this point. A very night-blind person would be deficient in the day also. Temporary night-blindness is due to some want of nutrition of the retina, and is often associated with scurvy. It is now a rare disease in this country. The ordinary varieties of night-blindness are due to disease of the retina.

Question—Does this failure (the permanent night-blindness due to disease) come on in middle life?—It varies; some are born with the disease which causes it, and with others the disease

comes on later. In most cases it gets worse. It is not due to the pupil not dilating, though a fixed small pupil does cause a slight degree of the symptom.

There is a group of cases of colour-blindness always associated with defective acuteness of vision, the peculiarity of which is that the affected persons see best by dull light, and cannot see nearly so well in bright light—"day-blindness with colour-blindness." The condition is due to disease occurring very early in life, and is stationary. It generally affects several members of a family, and the females as much as the males. Usually the colour-blindness is complete, and often total. Probably some of these cases have from time to time been taken for examples of ordinary congenital colour-blindness.

Evidence of Captain Macnab, of the Local Marine Board at Liverpool.

I am Chief Examiner and Secretary to the Local Marine Board at Liverpool, established under the Merchant Shipping Act. I supervise the colour-testing, and frequently conduct it myself; in fact, I examine more than anyone else. We have a dark room in which we take the candidates, and have the usual lanterns supplied by the Board of Trade, with the uniform slides. We place the man 18 ft. away from the light, and ask him the usual questions. We also ask him to name colours; if he succeeds in passing all these tests we give him a certificate, and, if not, we reject him.

Question.—What are the usual questions?—We use the usual shades, and ask the man to name them. They are the same colours as the Board of Trade use. Both officers and men are examined by me. They are examined on first entering, and afterwards. The officers generally come from schools, and are of the apprentice class; we also get a great number of men from large steamships—common sailors.

Question.—Are either the officers or men allowed to enter the service of the Companies without passing the prescribed examination for colour-vision?—Yes, anybody can go to sea without passing the colour examination. The last come because large steamships find it wise to have them tested. They come direct from the ships. As a general rule, some official from the docks, who has to look after the gathering of the crews, comes and brings a batch of men—quartermasters and sailors—with him. A quartermaster is simply a man who steers, and keeps the gangway. He wears the Company's uniform. He would often have to take the look-out duty. On an emergency, say, if a large number of the crew were down with fever or dysentery, they might take a lower class of men, who had not been examined for steering and look-out duty. When I was at sea it was customary to take "look-outs" from anywhere. The examination for colour-blindness had not then been instituted. It is usual now to submit men to be tested

for colour-vision in the best Companies, such as the White Star, Cunard, Guion, National, and Inman—all the large Atlantic Companies. They get no higher pay on account of having a certificate. They would not be admitted in the large Companies if rejected by me. Every officer who applies for a certificate of the coasting Companies is tested in colours. There are two different ways of applying to be examined; one, when a man applies to become an officer, and another, by which any one can come, without formality, by paying 1*s.* to be tested, and, if passed, certificated on the spot. If a man fails, he can come up again. We had one who came up four times. I have had cases where they have failed once, and afterwards succeeded, but this happens very seldom. I have seen a fair number of failures. From May 1, 1877, until December 31, 1889, in my own port, 12,272 persons were examined for certificates as officers; 90 failed, which gives 1 in 136, or ·73 per cent. for that class. I cannot tell in what proportion those men who had already passed were to those examined for the first time. In the figures I gave the same man is not counted twice; they are individuals, and I can say, speaking from memory, that I do not believe there were two people who had been failed at other ports and passed at ours. Prior to 1885 there was great diversity in the mode of conducting the examination, the appliances being different; Liverpool, before that date, was, I believe, the only port that had a dark chamber and a decent lamp; after that, the Board of Trade issued uniform lamps and glasses. With reference to the failures, there is another class showing a higher percentage than the officers, viz., those paying the shilling fee, principally quartermasters and forecastle hands. Since May, 1880, when the 1*s.* fee system began, we have examined 942; out of these, 34 failed. During the four years 1887–1890, when the records were kept more accurately, 22 out of 777 failed—a percentage of 2·83 Most of the applicants were rough seamen, with some few of the officer class who had failed before.

Question—Have you any explanation to give why a man succeeds after once failing?—Perhaps by getting the colours and being coached up. His colour sense might be improved, but I think not.

Question—Do you find many people ignorant of the names of colours?—That is one of the great difficulties I have never tried to solve; it is a scientific question. I have never tried with two lights at the same time, and asked the candidate to name them. I always conduct the examination exactly in accordance with the Board of Trade instructions. Men do appeal from my decision and go to an oculist; in fact, if ever I do fail a man, and he is young and possibly curable, I advise him to go to an oculist in order to ascertain whether he is colour *blind* and *in*curable, or colour *ignorant* and *curable.* I sometimes find in testing a man coming up for a higher certificate, that he fails the second time, although he has once passed. I do not trace this to any

peculiarity of vision, but I believe, in most cases, the first passing was a fluke.

Question—I suppose that if it is possible to pass by a fluke, the method of examination is not satisfactory?—I am not prepared to say that I think the colour test, as conducted at present, is unsatisfactory, if properly applied. If I have any doubt I always make a man repeat the names of the colours in his own language.

Question—I think you said something about crammers. If they cannot develop colour sense, how do they help the candidates? Is it by showing them the lamps, or using the apparatus?—I believe they provide themselves with a set of colours as nearly like ours as possible, or the same. I know one case of a teacher with a similar set. He would show a colour to the man who would say, perhaps, "it is red," and tell him that whenever he saw that which appeared to him to be "red" he was to call it "green." I am not quite satisfied as to the proper names to be given to all the coloured glasses we use. There are some you might perhaps be in doubt about if you had not been told the names. These are the confusion tints.

Question—Do you think there is anything beyond colour, any kind of perception, which would enable a man to distinguish colour?—No, I think not. I sometimes use the wool test, which consists of different coloured wools with a number attached to each. I give him a test skein, and tell him "to toss over all of this sort of colour." I apply this test to perhaps three cases in a year. I think the ignorance in naming colours is getting less. I believe many of the first failures were recorded because a man did not know the names of colours. I think it of supreme importance in our business to ask candidates the names of colours, and it is better than asking them to match colours, because the man must transmit the name of the light he sees to the officer of the watch, and if he gives the wrong name it might mean disaster.

Question—Your impression is that colour *knowledge* is as practically important as colour *vision*?—Precisely; only that the one can be acquired, but not the other. Something ought to be done as to vision; we have no authority to test for that.

Question—Do you take a man with weak eyes?—We cannot stop such a man going to sea, though he would not see in a heavy wind or rain.

Question—Do you think fog interferes with the lights?—Certainly; it takes from the carrying power, and turns a green light to white.

Question—Have you any means of explaining as to this to men joining the service?—No, it is not within our scope.

Question—Could you make any suggestion as to what should be used as a test for acuteness of vision and power of seeing at a distance?—Not beyond standing by the man, and ascertaining how he can see things at a distance. It would not be sufficient

for him to pass such a test once; he should be re-tested every ten years at least. Not many men come to me wearing glasses. They consider it rather *infra dig.*, and glasses would interfere with the discharge of their duty, being affected by rain, &c.

Question—What is your opinion on the practical importance of the question of colour-sight in the Navy and Mercantile Marine, and as to any facts which have come under your notice, that tend to show it is an occasional cause of disaster?—I have no statistics or cases on record, but it seems to go without saying that, if a man cannot describe colours, it may lead to disaster, and there may have been many disasters that could be traced altogether to it, although we cannot prove it. The importance of the question cannot be over-estimated. I know of no instances of collision or shipwreck where the colour-vision of the persons psssibly in fault has been tested in legal or other enquiries, but I know an instance of a man who was chief officer of a steamship and had been in the Company many years, and was promoted to the command of a large vessel, and then asked to get his certificate for colours. He tried at London and Liverpool and failed at both, and then realized the fact that he was hopelessly colour-blind. If the Company had not asked as to his colour-vision he would probably be at sea at the present time. That man had passed the Board of Trade examination in navigation and seamanship, but not for colour-vision. Another man I know of, who has failed in colours six or seven times, I have seen in command of a vessel with the Board of Trade highest certificate as an extra master, but he is unable to distinguish colours. He passed his examination for navigation, but his certificate is endorsed "colour-blind." The Board of Trade cannot forbid the employment of such a man. It is very unsatisfactory that a man who has failed to pass the colour test should command a vessel, and I should recommend legislation to alter this, as that is the only way it can be stopped. I think that beyond being able to distinguish red and green lights when they are together, a man should know the green, even if he could not see the red light, and many of these colour-blind people would be able to distinguish between red and green if they saw both together, especially if crammed up beforehand. The diminution of the inability to recognise green becomes of great importance. There are only two roads to go, and you must be either right or wrong. I sometimes find candidates call our green light white.

Question—Supposing they could distinguish on board ship a green light, by its appearing to them white, and the other red, would that be sufficient?—No, because they might mistake a steamer for a fishing boat. It is essential to be able to distinguish green as accurately as possible, and at as great a distance as possible, and if this power is diminished to a certain extent, danger may be apprehended.

Question—There is often no time for deliberation in forming a judgment?—No. Often the light cannot be seen until the vessel is

close, and one false move precipitates calamity. Stormy weather is at times the clearest, but often the condition of weather is such that a man, upon seeing the lights, is close upon the other vessel, and has very little time to make up his mind. That is, in fact, the normal condition of affairs round the British and American coasts. The man who can see green thoroughly and easily will have a larger margin for action. In the case of a man whose vision is imperfect, he would waste his time in making up his mind as to the colour, and pride would not allow him to call anybody to his aid.

Question—You have had large experience at sea?—Yes.

Question—Do you think red and green are the best lights?—The best up to the present, but we want a better green; it is too weak. It is apt to turn white in a fog. It does occasionally happen in enquiries that there is a difference in the evidence about the light shown, but I cannot answer from my own experience. I have never given evidence before the Admiralty Court.

Question—Would you recommend, in cases of collision, that an examination should be made as to the colour-vision of the officer in charge of the vessel?—Yes, whenever there was reason to doubt about it.

Question—Have you any knowledge of training ships?—I examine boys from the "Conway" and "Indefatigable," as officers, in one case, and sailors in the other. The examinations are systematic. If I reject them, it is a check against their further going to sea. Sometimes a boy does not want to go to sea after putting his parents to trouble and expense, and finds colour-blindness a good way to get out of it. I had one case in which a boy called every colour by its wrong name, avoiding the right name all round. I failed him, and told his people I did not think his colour-blindness was genuine.

Question—Then after receiving all the advantages of the training he might be rejected?—Yes, and he might be made a junior officer before he appears for the examination, and perhaps be in charge of the ship in fine weather. The authorities are very careful with regard to colour-vision, and reject a good many. I examine 40 to 60 of the "Conway" lads in a year.

Question—With regard to the "Indefatigable," supposing a boy was found to be colour-blind, would the authorities of the ship dismiss him?—No; he would be quite free to complete his education.

Question—Are you quite satisfied with the tests you use?—I believe they answer the purpose, though they will not tell whether a man is colour-blind or colour-ignorant. I think there is a very bad chance, practically, of a man passing the test who is colour-blind.

Evidence of Staff-Surgeon Preston, R.N.

I have had three years' experience with the testing for Colour-vision in the Navy, that is, the examination of recruits for the Marines, domestics, stokers, and boys; also of every class of officer entering the Service, at the Admiralty. In 1888, for which year I only examined a proportion of the cases, the total number examined was 2,935, in 1889 3,856, and in 1890 3,961. With regard to the Service, it is a matter of great importance that we should not have any persons either with defective vision or imperfect perception of colours, and with a view to that end a printed form is always forwarded to the parents or guardians of any young gentlemen coming up for naval cadetships, or assistant clerkships, recommending that previously to their educational examination for these posts by the Civil Service Commissioners, they should be medically examined by their own private practitioner, and special stress is laid upon the fact that the candidate would be unfit for the Service if affected with blindness, or defective vision, or imperfect perception of colours. [The Witness here handed in a copy of the form referred to, calling special attention to paragraph 4.] The larger number of those entering the Service, principally blue-jackets, stokers, and Marines, have nothing of that sort submitted to them, but they are subjected to a preliminary examination by a couple of Sergeants, before being passed on to me as medical examiner. The preliminary, or rough test, consists of the ordinary asking of questions as to bright colours on card-board. I may remark that I see about 3,000 men and boys a year at the *Rendezvous*, but there are nearly three times that number who come in the building applying to enter the Service, or raised by the Recruiting Sergeants; only one-third, however, come to me, the rest being rejected for some cause or other. With regard to the men—stokers, Marines, servants, and dockyard apprentices—I simply use the ordinary colour test. [Test board handed in.] Each person in succession has to cover one eye, and then a colour is pointed out, and he is asked what colour it is. If there is the slightest hesitation in replying, Holmgren's wools are used. That is the system which has been used for many years with men and boys, and I have found, as a rule, defective colour perception is hardly to be found among that class of people, doubtful cases being in nearly every instance due to colour ignorance, and appears to be confined to men and boys raised in the country recruiting centres of England and Scotland. In many instances these persons will confuse the brighter colours, yellows and blues; they understand green, but frequently, especially with boys raised in the Eastern Counties, where they are recruited from agricultural labourers, they cannot detect some of the test greens, although they will at once recognise grass-green with Holmgren's wools. I am speaking of boys from the country as contrasted with those raised in London or twenty miles round, of whom a large number come to us every day.

In the ordinary examination the candidate would be told to point out all the colours on the board. We find it necessary to state to the candidates that there are four simple colours—no crimsons, oranges, or violets.

In case a candidate fails to name correctly the colours on the board, we satisfy ourselves further by using the wool test. The men who are going to be examined have no access whatever to the test board, and to vary the positions of the colours we turn the board round. We carry our tests much farther in the case of officers, particularly naval cadets and engineer students, who are required to have absolute normal vision and colour vision, each being examined separately by Snellen's test, supplemented by flags and wools. They stand at a distance of 16 feet, and are shown each of the flags separately, and have to name them in quick succession, tested with either eye. That is the first test, and the next is Mr. St. Clair Buxton's marine telechrome. [The Witness exhibited this apparatus, and explained its use.] The glasses in this lantern are used at the same distance as the flags (16 feet), with red, blue, violet, green, and white lights in quick succession, and with the fogging apparatus, which is simply a piece of glass fogged on one side, with no lens whatever. Supposing a candidate mistakes between red and green, we take a further test. The candidate is allowed to wait while the rest of the examination is proceeded with, and is then re-examined on the doubtful point, as it is absolutely necessary that an executive officer should discern at once every coloured flag, either of our own or foreign nations. In several cases the Medical Director-General has allowed a young gentleman to come up a second time for examination one or two days later, but I have looked through the records and find they are never successful when once defective colour perception has been detected.

The figures giving the proportion of candidates rejected are as follows:—In 1888 there were 214 examined for Naval Cadetships, and of those, one was rejected for inability to distinguish greens from browns, and another was found ignorant of the names of colours. It appears, however, in the records that upon being examined subsequently the same day he was passed. In 1889, out of 293 examined, there were 1·02 rejections for defective colour perception. Of these one was rejected for confusing greens and browns; one was absolutely colour-blind; and one in the immediate perception of colours was uncertain. In 1890, 305 naval cadets were examined, the percentage of rejections being 1·31. Of these one was rejected for inability to distinguish between greens, reds, and browns; and three were rejected for being unable to distinguish green from red. These were boys whose parents had received the warning as to defective sight; but, as a rule, parents do not care to go to the expense or trouble of a medical examination beforehand by their own doctor.

Although I have laid great stress upon promptness in replying to the questions in examination, we do not reject candidates for

want of promptness. If there is the slightest doubt we re-examine, always testing such cases with Holmgren's wools before finally rejecting them. Naval cadets and engineer students have four examinations in colour-perception before they are declared unfit.

The conclusion in the case I referred to as totally colour-blind must have been arrived at in the ordinary way by Holmgren's wools, the flags and buntings. I may mention that if boys, when sent to the training-ships from our *Rendezvous*, are suspected with regard to their colour-perception, it is reported, and they are, when on the ship, tested by night as well as day: by day with the telescope up to, say, the distance of a mile, and at night-time with the coloured lights at the full length of the ship. [The Witness here exhibited a specimen of the Admiralty green glass, as used in the lights, and explained with reference to it.]

We never find anybody who can distinguish the Admiralty green who cannot distinguish a greener green. Where candidates persistently confuse red and green in the lantern, but sort the wools correctly in using Holmgren's test, it is the fault of the lantern not being sufficiently green. Speaking of colour-blindness which is not congenital, I should say that all the naval cadets to which I have referred were, as far as our registers show, rejected for colour-blindness which was congenital, but I should have difficulty in getting further information upon that point.

We have no records of men with normal vision among whom colour-blindness has been brought on by disease.

Naval officers are never examined after their appointment, and, therefore, they might be suffering from tobacco amblyopia; but naval cadets and engineer students are not allowed to smoke until they are eighteen years of age, and on the *Britannia* they are, of course, constantly being examined with colours.

When they have once been passed into the Service they would not be examined again systematically; but if there was any suspicion as to colour perception, they would be examined by the medical officer of their own ships, and invalided, and if found to have defective colour-vision, be removed from the Service.

There have been a few cases of blue-jackets who, upon offering themselves for rating as signalmen, were rejected as colour-blind, but upon closer examination it has been found to be due to defects of the accommodation of the eye.

This would be brought out in the following way:—Upon a man being examined for signalman he would be required to read hoists of 20 or 30 flags at once, and upon being asked by his examiner what a flag was would answer "w" instead of "q," and upon reference it would be found he mistook red for green. This might be due to defective vision rather than defective colour perception, but this would come out in examination at the shorter ranges.

All men are examined, as every man is a look-out man more or less. All pass a course of musketry and gunnery instruction,

and before this are medically examined to test their power of vision, because if this were not absolutely normal the training would be time thrown away.

They are examined by the officers of their own ships by the method laid down in the Queen's Regulations: coloured flags supplemented by Holmgren's wools.

I do not know of any case of an officer becoming colour-blind through disease. The defects that are found are generally those of accommodation, and occur primarily with officers about 45 years of age who are presbyopic.

To the best of my belief, there is no officer in the Service at the present moment at all defective in colour-vision; that, I believe, is so with regard to the executive branch, and engineers who are in charge of the machinery of torpedo boats. I do not think that there are any Marine or medical officers defective in colour-vision.

I know of no cases of collision, where there has been a court-martial on the loss of a ship, in which any doubt has arisen as to whether it occurred through inability to read the signals, except that of the *Iron Duke* and the *Vanguard*, where the look-out man was said to have been myopic. In such a case the question would undoubtedly be thoroughly gone into, because it would be the sole defence of the man.

The look-out men are put through the card test as boys, and are for five years after undergoing a constant test by their instructors, with flags and bunting, from a few yards to a mile, and with the telescope as well.

If a man was wrong in his signals, he would be detected, and examined by the medical officer, and then sent to hospital for further observation.

With reference to the statistics as to recruits in the Marines, I have looked back for six or seven years, and find none rejected as far as the Medical Officer was concerned, but they have been previously sifted by the Staff-sergeants, who examine for colour- and form-perception, using Holmgren's decided colours. I only see one-third of those who come up, the Staff-sergeants having probably rejected the rest.

No recruits who have passed the wool test are found to be inefficient subsequently among Marines, domestics, and artificers; but a small percentage, and with boys but an infinitesimal number, failed subsequently. It often happens that a boy who passes our test in London, and finding a life on board ship perfectly new to him, gets discontented, is told by somebody that by saying he does not know what certain colours are he may be sent to hospital, and invalided out of the Service; but they are examined at the hospital, and in all cases returned to their vessels.

Evidence of Dr. George Lindsay Johnson.

The Chairman: You are aware, perhaps, Dr. Johnson, that this Committee is investigating the general subject of colour-blindness. We gather that you have given your attention to the subject, and should be glad of any information you can give the Committee as to your experience of practical testing by various methods?—I am acquainted with most methods. I have used the spectroscope, and recently a simple form of Captain Abney's method. I have also had a little experience with Donders' method. Some of my testing has been with the spectroscope with a graduated circle, in which you read off the point where the spectrum appears to the patient to end. It is only with red-blind and green-blind cases I have had much practical acquaintance; although I have had one violet-blind. In using the spectroscope, I ask the patient to fix the point where the spectrum appears to end, and read it off on the scale, to see if I can get an improvement benefiting the patients who are red-blind, and in order to practically measure the improvement under special treatment. I may say with coloured wools or ordinary reflected colours I do not think it is so easy to ascertain whether patients make a definite improvement as by measuring with a Vernier's scale, with which the exact limit to which the red end extends can be made out.

Some patients find a difficulty in fixing the exact limit to which the red extends; but, as a rule, with intelligent patients, they can fix it pretty definitely.

The source of light I have hitherto employed has been a candle at Moorfields Hospital, and a paraffin lamp at home.

I find there is a variation according to the light used, and to prevent error on this account I have always used with the same patient the same source of light. Sky-light gives different results to candle-light, and also with regard to the fields of vision.

In taking fields of vision I generally use Dr. Priestley Smith's perimeter, which I have modified somewhat myself. I am not quite satisfied with the dead or pigment colours, but adapt to the perimeter an instrument for taking fields of vision, which I have had constructed for use with a 2-candle power incandescent electric-light.

[The Witness handed in a diagram illustrating the apparatus referred to, and explained with reference to its use.]

Question.—Does your experience go to prove that the spectrum colours all disappear at the same angle as Landholt holds, or do you find a difference in the disappearance according to the intensity of the light employed?—Yes. I am certain the difference is in accordance with the intensity of the light. I have not got figures at present with regard to the exact point where the colours stop with the spectrum, and do not think my figures would be of much use, unless interpreted by Fraunhofer's lines.

With regard to results obtained in increasing the sensibility of the eye to red in red colour-blind cases I have had a patient who came to me originally at Moorfields, who had been rejected by the Board of Trade because he could not see bluish-green or red lights on board ship. He was extremely colour-blind with regard to red, the red colour being shortened nearly up to the orange, so it occurred to me that acting on the supposition that in his case the trouble was probably central and not peripheral—for I could find no change whatever in the disc—I got him a pair of goggles so as to completely exclude all daylight except what filtered through the best red photographic glass with which they were fitted. I told him to wear these goggles the whole day long until he went to bed at night, not taking them off until the lights were out. He followed my instructions, and I tested him every consecutive month with Holmgren's wools, and noted on a list the colours in which he made mistakes. At the end of a month I found a considerable improvement, and at the end of three months his colour-vision was nearly perfect, being wrong in only three out of forty, whereas at first he was wrong in thirty out of forty. I sent him again to the Board of Trade for re-examination, and they found he was so much better than before that they told him still more, he might come to them again and they would grant his certificate. He went up again and passed completely in the red and green glass test with the lantern, but failed on the card test in the light pink and light blue. The last time I heard he had got the post of mate of a vessel trading between London and the Netherlands.

He wore these coloured glasses up to the time he passed. He said it was a great trouble seeing everything red, but insisted on keeping to them, notwithstanding the inconvenience; and upon testing him with the spectroscope—which is the only absolute test we possess—there certainly was an improvement in his vision as far as the extent of light towards the red end was concerned. I asked him to define red, but from what I could gather he had always had congenital colour-blindness, and it was very difficult to say whether his sensation of red was the same as ours. It is my firm conviction that the continual stimulation of some part of the conducting fibres or sensorium—whether peripheral or central—of red, awoke a faculty of perceiving something which may be called red. I did not try whether his colour-blindness was central, nor whether he relapsed after leaving off his glasses, but will make enquiries.

I tried with two other similar cases, which got a little better, but afterwards they gave up the goggles, saying they could not see well enough to go about with the glasses. I do not think the case I have mentioned was due to tobacco, as the man hardly ever smoked, and his vision was very acute, being $\frac{6}{9}$ths, or one line below normal, in either eye.

With regard to detecting colour-blindness by the ophthalmoscope I may say that I have strong reasons for believing there

are two forms of colour-blindness, viz., central or cerebral; and peripheral, or connected with the optic nerve, as in retro-bulbar neuritis, or in the retina itself, or the choroid. In those cases in which colour-blindness is congenital I can detect absolutely nothing with the ophthalmoscope which would lead me to suppose that the patient was colour-blind. I can, on the other hand, exhibit a number of diagrams showing marked changes in pathological colour-blind cases. The portion of the disc affected is more extended, and there is that wedge-shaped triangle on the outer side of the disc which is so characteristic of tobacco and other narcotic amblyopias, only in these cases it is generally more extended.

[The Witness here handed in a number of charts illustrating cases of colour-blindness, and called special attention to a pale spot on the retina which was characteristic of such cases.]

I have brought with me a patient who has perfect colour-vision with one eye, while in the other she has no colour whatever. She describes the appearance of the spectrum as being like a grey smear. I cannot find that she has any perception of violet.

Question—I think you said you had another point to bring before the Committee?—Yes. I have some information, the result of two or three years' study, which I am not sure exactly regards this Committee, but I have made some experiments showing that if you place glass in front of the eye so as to wholly exclude daylight as far as possible, and have glasses made so that only the blue-violet end of the spectrum passes through, cutting off orange and part of the yellow, the field of vision for white, if contracted, will after a week become enlarged to normal, and that holds good with some cases of detachment of the retina. I was induced to make a large number of experiments with rabbits to ascertain the reason. They were kept in a hutch-like photographic chamber so that no other light than that through the red or blue glass could reach their eyes. After a certain time they were killed and put in a black bag, their eyes being fixed in osmic or nitric acid. I found a distinct anatomical difference between the retinas of the animals under the different glasses; and these differences come under four heads:—

Firstly, in the animals kept in the blue light the rods of the retina adhere much more closely to the little processes of the hexagonal prism, so that the retina cannot be easily detached after death. Secondly, the pigment under blue glass is increased. Thirdly, the retinas take up fluid more easily than they do in the opposite colour; and lastly, not only do they stain much better in the staining fluids, but are also more developed, and seem to increase and multiply more than in the red glass. These four points hold good for all rabbits. It seems to me that animals kept under a constant source of blue wave-lengths have certain changes effected in the retina differing from those under the red end of the spectrum, and that possibly may account for the

reason that I find the field of vision increased in almost all cases of patients whose field of vision has been contracted beforehand, if kept for a length of time wearing the goggles I have referred to. I am not prepared to say whether if the rabbits are exposed to ordinary light the retinas would return to their former condition.

Question—It is said that photographers suffer from working constantly in a red light; do you think that is so?—No; but I think they suffer from bad ventilation, as a rule.

Question—You say that after death there is more difficulty in the detachment of retina after keeping the animals in a particular light; would that have any application to colour-blind cases, or point to any cure?—I cannot positively state an instance of a detached retina going back, but in almost all cases of slight detachment of the retina, where the field of vision has been cut off over a certain area, that area has been considerably increased after wearing blue glasses, except when separated by effusion and forms of umbrella detachment, when the retina becomes, as it were, bleached.

Question—Have you had any experience with progressive atrophy, as to whether persons suffering from it have a lack of colour-sensation?—I find their field of vision, both for white and for colours, is extremely contracted, and that nothing I have ever tried for them has done the slightest good. I have taken the field of vision for a great many atrophies, and find blue is the last colour to disappear, as well as the most extended. I have never found a patient with a blue field and no other, but I have found blue extends further than any other. According to Herring, the blue and yellow field ought to be co-terminous, but I do not find that to be the case.

Evidence of Dr. Edridge Green.

The Chairman: I believe you have paid a good deal of attention to the question of colour-vision, and perhaps you will be so good as to describe to the Committee the methods you have used in your colour tests, and the general character of the results obtained?—[Witness handed in a diagram illustrative of psycho-physical colour-perception, and explained as follows]:—

The theory is that the perception of colour is a perception of difference; colours are confused by the colour-blind, not because of any loss of substance, but because the individual cannot perceive any difference between the rays of light included in a portion of the spectrum which appears monochromatic to him. The size of the monochromatic band varies with the individual. A person who has very defective colour-perception has a monochromatic band so wide as to include several colours which are easily distinguished by a normal sighted person.

In some cases seven colours are seen, and then the seventh colour appears at the point where it should appear by theory. In the first degree of colour-blindness only five colours, or points of difference, are seen in the spectrum; in the next degree four; in the next, three; then two. Then a neutral band appears at the blue-green junction, and this increases in size in different cases until total colour-blindness is reached. Therefore, the vision of the normal-sighted being hexachromic, the vision of the colour-blind is pentachromic, tetrachromic, trichromic, or dichromic. It will be noticed that the greatest difference is to be found between the 3-unit and the 2-unit cases of colour-blindness, the primary colours for each being quite different. The two primary colours for the 2-unit are yellow and blue, and they each represent half of the spectrum. In the case of the 3-unit the three primary colours are red, green, and violet. Red combined with green forms yellow; violet combined with green forms blue; so it is evident that these colours occupy the positions which I theoretically allotted to them.

The above refers to the number of approximate psychophysical colour units. An approximate psychophysical colour unit is a portion of a physical series which contains physical units that are not easily distinguished from each other, and are so much alike as to be called by the same name. An absolute psychophysical colour unit is a portion of a physical series which contains physical units that cannot be distinguished from each other even under the most favourable circumstances. It will be seen that an approximate unit contains several absolute units, but in each case the similarity between them is greater than the dissimilarity; for instance, there are many hues of red, but the character of redness enables them to be classed together.

The other chief cause of colour-blindness is shortening of one or both ends of the spectrum. This is probably due to some retinal defect, as neither light nor colour are perceived at the shortened end. It is distinct and separate from diminished psychophysical perception, which is due to defective size of the colour-perceiving centre in the brain.

Question—How would you establish the six or any other number of colours with the person you were examining?—A person who sees six will at once say so. I test with a spectroscope provided with two shutters in the eye-piece, showing the examinee in the first instance red, orange, yellow, and yellow-green, because these are of nearly equal luminosity. I make him indicate the junctions of the colours; any colours can be cut off with the shutters, so that the person examined is not able from one colour to guess the others. I use an ordinary spectroscope (one prism), provided with shutters, as explained. The actual procedure in using this test is as follows:—I ascertain where the spectrum commences, where it terminates, what colours are seen, and where the junctions of the colours are, the

patient using the shutters until he is satisfied that he has obtained the correct junction.

Question—If you had say five points of difference, would they always come exactly in the same place?—Yes; such a case would not see orange as a definite colour.

Question—Would a patient recognise no orange, supposing a single colour was suitably chosen and distributed over a sensible field, and beginning from the sodium line?—A 5-unit case usually objects to the term orange; he would probably call it reddish-yellow.

I have not come across any 1-unit cases; that is the only one on the diagram that is not drawn from my own experience. I should explain that the diagrams are all drawn the same lengths, to demonstrate the psychophysical diminution of colour-perception and not the shortening of the spectrum, in which case another effect is produced, viz., the junctions of all colours are altered; a 2-unit, with shortening of the red end of the spectrum, puts the junction of his two colours nearer the blue than a 2-unit with an unshortened spectrum.

Diagram No. 6 shows a transition from red to violet; in such cases there is no neutral band in any part of the spectrum. The one colour passes into the other without any definite intermediate point.

In examining for scientific purposes the spectrum would be the first test, afterwards wools, pigments, or lights, in accordance with the spectrum examination.

Question—Would it not be easy to coach a person for this?—No, because he would never be able to hit off the exact junctions of the colours.

Question—Which class of the colour-blind would you consider as representing dangerous cases for signalling purposes?—I. Those who possess a psychophysical colour-perception with three or less units. II. Those who, whilst being able to perceive a greater number of units than three, have the red end of the spectrum shortened to a degree incompatible with their recognition of a red light at a distance of two miles. III. Those who are affected with central scotoma for red or green. The 3-unit would be unsafe, for though he would always recognise red and green, even to the lowest degree of luminosity, he would confuse yellows, especially dark yellows, with reds and greens, and generally call them reddish-greens; in fact, yellow has been described by such a patient as being of the same colour as a red clover field in full blossom. The 2-unit cases and below are absolutely dangerous. The 5-unit and 4-unit are safe.

[The Witness here handed in his Pocket Test, and explained its use, Captain Abney being asked to pick out all the shades of orange.]

Question—Would you describe a person as not having distinct orange perception, who could not mark out the definite regions on the spectrum bounded on one side by yellow and on the

other by red ?—Yes; I should describe such a case as not seeing orange.

Question—I understand Dr. Brown's test comes out under your patronage; may we therefore take it that you approve of that method?—Not at all. A medical man might roughly test with it.

Question—Do you test by nomenclature or matching?—By nomenclature, combined with matching. Many normal sighted persons fail with Holmgren's test because they think a shade is a colour, paying as much attention to the one as the other; if you say "I want you to pick out all the greens," you give them something tangible to go upon. If a person in picking out twenty or thirty greens also picked out half-a-dozen reds, it would be certain he was colour-blind; but if he has to match a green wool he might pick out with the greens a light brown, and not be colour-blind at all, and the error could be rectified by explaining to him that the colour he selected was greenish-brown, reddish-brown, or yellow-brown, as the case might be. With Holmgren's test, under the same circumstances, he would have failed. This might be confirmed by asking him to classify the whole 150 colours.

Question—Do I understand you that a normal sighted person might pick out brown instead of pale green?—Yes, because he might pay more attention to shade than to colour. In testing practically I should first use the Classification Test. I should not begin with the spectrum with a practical test; it would not be convenient, and persons would object to it.

[At this point the witness was asked to apply the Classification Test to Mr. Rix. Mr. Rix was first requested to pick out all the shades of orange he could see, and in so doing he selected two skeins of wool of a decided light green. Dark blue and violet were matched as being of the same colour. In matching reds, a reddish-brown was picked out, but described as having more blue in it, and blue-green was sorted with the drabs, and referred to as being brighter.]

WITNESS.—The Classification Test is used in order that inexperienced examiners might not have to depend upon the Lantern Test—in which not more than twenty answers are required. I do not use the Board of Trade colours with that test, but my own. The person examined should be able to distinguish between the red, green, and white lights, either alone or modified with the neutral glasses.

Question—Why do you think it necessary to have a preliminary test to this?—In order that an inexperienced examiner may feel certain that the mistakes made with the Lantern Test are not due to colour ignorance.

Question—Have your investigations been with pathological or congenital colour-blind cases?—With both.

The principles upon which I examine are as follows:—The first principle which guided me in the selection of colours may be

illustrated in the following way:—Let us take an ordinary 2-unit colour-blind, and, having given him the set of wools belonging to the Classification Test, ask him to pick out all the reds. On examining the pile of wools selected as red, it will be found that the majority are red, but in addition there will be some browns, and yellow-greens. If he be then told to pick out the whole of the greens, the greater number of those selected will be greens, but there will be also greys, browns, and reds. In each case it will be seen that the majority of wools are of the desired colour.

If another 2-unit colour-blind be examined in the same way it will be found that, though he may not make exactly the same mistakes, he will in all probability pick out the same greens to put with the reds, and the same reds to put with the greens. The same result will be obtained if the colour-blind persons be asked to name a large number of colours. They will in most cases name the colour correctly. It will be noticed that the greens which were put with the reds when classifying the colours, will be called red in naming them. It is evident that the same idea has guided the colour-blind in each case.

This shows that, though a person may be red-green blind, he is not absolutely red-green blind in the sense of being totally unable to distinguish between the two colours. This is what we should expect, as the red and green are included in an approximate, not in an absolute psychophysical unit. The fact that they are actually judging by colour may be demonstrated by giving them coloured materials of different kinds, or by asking them to name a large number of coloured objects. To a person with a spectrum of normal length and no neutral band in the blue-green, it is necessary that the colours, to be considered as identical, must be included in an absolute psychophysical unit. One of the most definite signs that persons with a neutral band in the blue-green have a more defective colour-perception than the ordinary 2-unit, is that they will put together as identical a red and green which are distinguished by the ordinary 2-unit. In addition to this, they will mistake the reds and greens which have been confused by the ordinary 2-unit.

It will be seen that if we take a 2-unit and ask him to name a number of red and green wools, in the majority of instances he will name them correctly. But as, almost invariably, the same wools are chosen, for all practical purposes the same result would be obtained by asking a person to name a few of these wools. What more decided and brighter greens could we have than Nos. 76 and 94 of my Pocket Test? yet these are two of the greens which are called reds by the 2-unit. We should have accomplished as much by asking a colour-blind person to name Nos. 76 and 94 as if we had asked him to name a large number of greens. The colours in a test should, therefore, be those which the colour-blind are particularly liable to miscall. At the same time, their nature should be unmistakable to the normal sighted.

My second principle is that a colour-blind person will name

colours in accordance with his psychophysical colour-perception, and thus show distinctly to which class he belongs.

The third principle is that colours may be changed to colour-blind persons whilst leaving them unaltered to the normal sighted.

Fourthly, the phenomena of simultaneous contrast are much more marked with colour-blind than with normal sighted persons. Two colours not changed to the normal sighted, on being contrasted, apparently alter considerably to the colour-blind.

These tests are described in full in my book on Colour-blindness and Colour-perception in the International Scientific Series.

Question—How would you proceed, supposing you were asked by a railway company to test 500 men? What arrangements would you make, and how long a time would the examination be likely to occupy?—I should examine each separately, taking care that the others did not look on. I should first examine with the Classification Test, and then put them through the Lantern Test, taking twenty answers in each case. The process would only take about five minutes for each man, with even one examiner, because one man could be going on with the classification whilst another was being examined with the lantern. I allow ten minutes for each man, because I think that it is a great mistake to hurry, or be in any doubt about a case.

Question—At that rate it would require 80 or 90 hours to examine the 500 men?—It would mean a considerable expenditure of time. You want to know if a man can distinguish between red, green, and white at a distance of two miles. You might commit yourself to the Lantern Test alone; but if one man was sorting the wools while another was being tested with the lantern, it would take little or no longer to employ both tests, and in rejecting a man the double test would be conclusive. I regard both tests as desirable, and the Lantern Test as essential, for that would detect scotoma, whereas the Classification Test would not.

Question—You said you tried a progressive atrophy case, and that he was a 2-unit man, and his junction at about the E line?—I cannot say without referring to my note-book, but he saw one part of the spectrum as whitish and the other blue, only seeing those two colours. [Capt. Abney said:—When I tried him the junction of the white was at λ 4·733, between F and G, and nearer to F, there being a sudden commencement of blue at this point. At 26·5 of my scale he saw a little blue, and at 26·75 no colour.]

The essential part of my theory is that psychophysical perception is due to the brain and not to the retina. The theory I have formed is that the visual purple is liberated from the rods by light, and forms a photograph at the back of the retina, and that the cones only act as transferring organs of the percipient fibres, transferring the impression of the photograph to the brain.

Question—Is not that something like Kühne's theory?—No; he took a different view, the objection to which was that there are no rods in the yellow spot; but according to the theory I have advanced it would be essential that there should be no rods at the yellow spot.

Evidence of Capt. Angove.

The Chairman. You are, I believe, the Captain Superintendent of the Peninsular and Oriental Steam Navigation Company?—Yes, the Marine Superintendent.

You are probably aware that this Committee is investigating the question of colour-blindness, and the precautions taken by steam-shipping companies and railway companies against accident from this cause; we are therefore anxious to hear what is the practice of your company with regard to examining officers?—One of the Managing Directors and myself first examine applicants with regard to objects at different distances from the windows, then the Company's Medical Adviser examines them with coloured wools and for distance sight. They have to arrange and name the different colours. The vision for distance is tested by placing the candidate at the regulation distance from ophthalmic large test types, and the near vision by corresponding small type. It is required that both eyes should be equal to the average of good sight. We find, I am sorry to say, a great number who are not up to the standard, and often have to reject candidates in consequence.

Question—That is the procedure for candidates for the posts of officers?—Yes, but there is also a Board of Trade examination before coming to us which they have to pass in getting their certificates for various grades of officers.

Question—Do you take pupils in your Company?—No, not now; in former years we had two training ships, but they have been discontinued, so that every officer must now hold the Board of Trade certificate, which includes colour-vision. We often find candidates deficient, and reject a great number for defective sight.

Question—Then that points to the Board of Trade examination being unsatisfactory?—Yes, I do not think their examination is sufficiently rigid.

Question—Would you in your examinations reject candidates both for colour-vision and form?—Yes; and we give equal attention to weakness of sight with regard to seeing at long distances.

Question—Do you give rejected candidates a second opportunity?—No, but some go to an oculist's on their own account, and we get a special report from him. The weakness may be only of a temporary nature. I attribute a great number of cases to over-smoking with young men. We have traced several

instances to that cause, and where the smoking has been discontinued the sight has improved.

Question—You said, I think, that in deciding for colours candidates have to arrange coloured wools?—Yes, they have to arrange greens, reds, &c.; they are all mixed up, and the candidates have to pick them out, compare them, and sort those of the same colour. It is really the Holmgren test.

Question—After a candidate has been passed and admitted into the service, are there any tests subsequently applied?—Not unless he is reported by his commander. We have found some men colour-blind after being some years at sea and in possession of Board of Trade certificates. There was one instance of an officer who was found to be quite colour-blind, and was consequently tranferred to a clerkship in the office. He had been passed by the Board of Trade, but not by us, as at that time we accepted Board of Trade certificates, and did not have our own examinations for sight. Since we have found these various cases, we have realized the necessity of having our own examination, and have called the notice of the Board of Trade to the fact. I have with me copies of correspondence which passed between Mr. Barnes and Sir Thos. Gray pointing out the fact to him that we have to reject so many young officers for defective sight. The Board of Trade admit the test should be more severe.

Question—Have you never heard of cases in which the failure of the eyesight of officers has led to accidents?—I cannot say that I have. The case of the gentleman I have mentioned who was colour-blind was discovered, I suppose, simply from his being on the bridge of the vessel with another commander. He rose to be a chief officer before it was discovered. His weakness might have led to accidents.

Question—Have you any statistics drawn up with regard to these cases?—Not beyond these facts that we are rejecting young officers. In a letter dated the 9th April, 1889, to Sir T. Gray, Mr. Barnes referred to the many painful interviews with candidates who have wasted six or seven years learning their profession with the expectation of entering our service, and who, when the height of their ambition is about to be realised, find we are compelled to reject them on account of defective vision. In one case a young fellow, with a new Board of Trade certificate, who seemed a desirable man, could not read letters 2 to 15 inches long 20 yards from the window. In the last two years a considerable percentage of the candidates passed by us, as in other respects satisfactory, have failed in the sight test for colour or distance vision; mostly the latter.

Question—Do you find any improvement in the Board of Trade examinations since you have written to them upon the subject of colour-vision?—I think they are getting more particular, from what I can learn from young officers.

Question—Another branch of our enquiry is as to what precautions are taken as to the vision of seamen?—We accept

the Board of Trade certificate which they have to obtain. Without that we do not admit them. We have no test for Lascars. Some of our ships are manned by them; but the look-out men are always Europeans.

Question—That leads to the question of what are the responsibilities of look-out men?—It is a very important position, as they have to report everything seen by them to the officer in charge of the bridge; though often he sees objects before the look-out man, because he is in a higher position. It would be a bad thing if a look-out man was colour-blind.

Question—Does he only have to report "light ahead?"—Yes, or on one bow or the other; but he usually gives the signal with a gong, striking once if the light is right ahead, twice if it is on the port bow, and three times if on the starboard bow. He would very likely sing out, "green light," or whatever it might be, after striking the gong.

Question—Do you think the question of colour for the seaman important?—Not so important as for the officer.

Question—Supposing a vessel suddenly emerged from a fog, and a green light was seen on the port side and a red one somewhere else, would it be necessary for the look-out man in that case to say where the green and red light was?—Yes, it would be of great importance if he could.

Question—Supposing you had a foreigner, say a Welshman, who only knew Welsh, if he sang out the Welsh for red, which I believe is very like green, would it not be nearly as bad as if he were colour-blind?—Quite.

Question—And then take the case of a German seaman, for instance?—They all understand the colours. We should not ship him unless he had sufficient experience to know port from starboard and red from green, and was familiar with the English expressions for them.

Question—If you rejected him he could probably be shipped somewhere else; I presume you think the company or vessel might be at a disadvantage by shipping a man who was colour-ignorant although not colour-blind?—Yes.

Question—There seemed to be some doubt as to whether it was necessary for the look-out man to have perfect vision for colour and form?—It is most necessary (especially when in close proximity to a light, and the vessel perhaps altering her course). The look-out man should sing out immediately red or green light, as he may see it before the officer on the bridge, should a sail, for instance, happen to hide it from him.

Question—Do you know of any accidents that have been traced to absence of colour-vision?—I cannot say we have traced one actually to it, but we have had suspicions about it, and they naturally have led to special enquiry. In addition to the case I have mentioned there was another officer, a second officer, who, upon being questioned, was found to have his sight affected. In all such cases we insist upon their leaving.

Question—With regard to shipping Lascars, you say they do not take look-out duty?—They are a sort of assistant look-outs; they have no responsibility, as that always rests with a European. There are generally two on the look-out together, one European and one Lascar. As a rule they pick up a certain quantity of English, and they have very good eyesight, though I do not know as to their colour-vision; they do not go through any examination. Between the two, that is to say the European who has picked up a certain amount of Hindustani, and the Lascar a certain amount of English, there is an understanding between them.

Question—Do you think the precautions taken by the Board of Trade with regard to seamen are sufficient?—I would not say they are stringent enough. They examine by means of coloured lights in a narrow passage, and the man has to call the colours. The naming of colours I consider to be a right test, as well as matching colours, on account of cases of colour-ignorance. Seamen might not know the names of all colours possibly; but as long as they name red and green without mistakes, that would be sufficient.

Question—Have you ever met anybody who called a light a black light?—No.

Question—Do you consider that the coaching for the Board of Trade certificate, which is known to be practised, might be the cause of the comparative ease with which defective colour-vision men get the certificate?—I do not think the coaching is sufficient to account for it.

Question—Does your examination include coloured lights?—No, coloured wools only; the Board of Trade use the coloured lights.

Question—We may take it that you examine all the officers, and you accept the Board of Trade shilling certificate for the men?—Yes, we always insist upon that. All officers, from the first to the fifth, go through our tests.

Question—Have you any suggestions you would like to make with regard to the tests?—I think not, except that the Board of Trade cannot be too severe with their examinations, and a little strong pressure might be brought to bear upon them in this direction. We should be glad to have the examinations made sufficiently reliable to relieve us of the necessity of doing what they ought to do.

LETTERS RECEIVED BY THE COMMITTEE BEARING ON THE ENQUIRY.

"No. $\frac{7680}{1602}$

"Pall Mall,
"*27th May*, 1890.

"SIR,—I am directed by the Secretary of State for War to acknowledge the receipt of your letter of 20th instant, and to acquaint you in reply, that tests for Colour Vision are invariably used in the case of all candidates presenting themselves for Commissions in Her Majesty's Service.

"Holmgren's wools being the most convenient, are employed in a systematic manner to detect any defect.

"The plan consists in making the candidate match certain test colours from the heap of wools.

"I am to add that recruits are not tested for Colour Vision.

"I have the honour to be, Sir,
"Your obedient Servant,
"RALPH THOMPSON.

"The Secretary to the Committee,
"Science and Art Department,
"South Kensington."

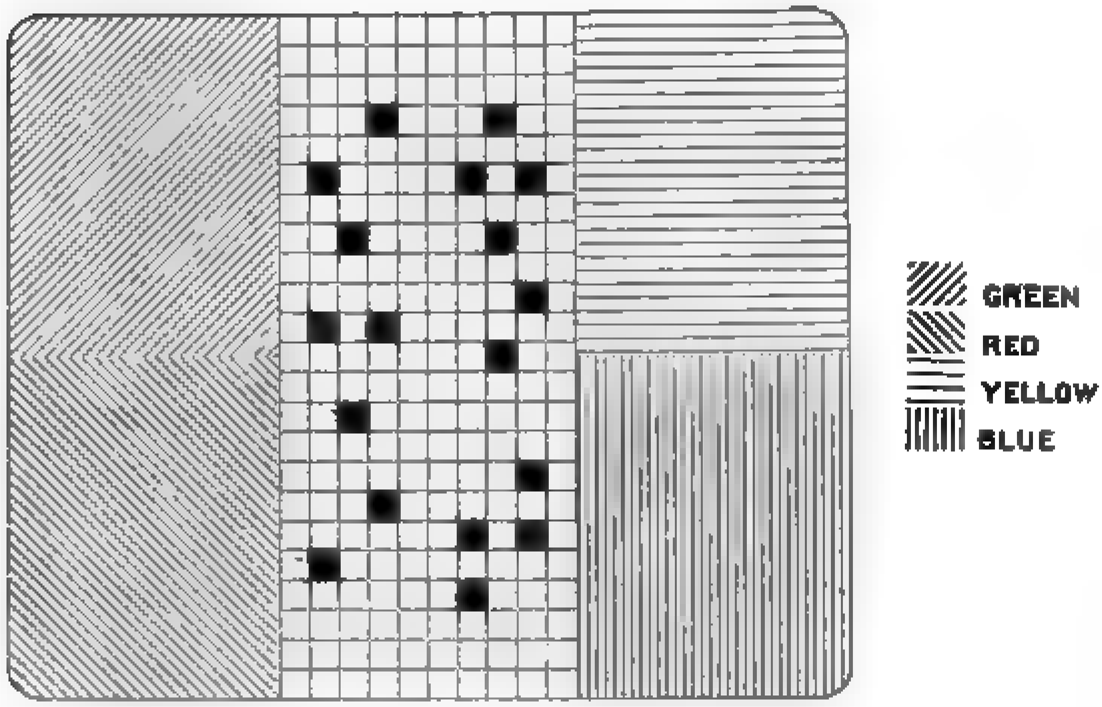
"No. $\frac{7680}{1604}$

"Pall Mall,
"*5th June*, 1890.

"Sir,—I am directed by the Secretary of State for War to acknowledge your letter of 2nd instant, and in reply to acquaint you that there are no statistics regarding Colour Blindness compiled in this Department.

"I have the honour to be, Sir,
"Your obedient Servant,
"RALPH THOMPSON.

"The Secretary to the Committee,
"The Royal Society,
"Burlington House, W."



"*Description and Instructions.*

"(Modified after the Regulations issued from the Horse Guards, by Prof. Longmore, in 1868.)

"Each test dot on this card is one-fifth of an inch square, and corresponds, at a distance of 15 feet, with the bull's-eye, 2 feet square, at 600 yards, required by order to be distinctly seen by every acceptable recruit.

"*Men.*—With perfectly acute vision these test dots ought to be clearly visible in full daylight at 19 yards.

"1. Expose the card in full daylight at a distance of 15 feet from the candidate.

"2. Examine each eye separately, taking care that the unused eye be merely covered, not pressed upon or closed.

"3. Vary the number and position of the dots by covering some of them and moving the card.

"4. Test each eye as to recognition of colour."

"Civil Service Commission,
"Westminster,
"*28th May*, 1890.

"Sir,—In reply to your letter of the 20th instant, I am directed by the Civil Service Commissioners to inform you that in the medical examinations conducted by them no tests of Colour Vision are employed, except in the case of the examination for the India Civil Service, where the tests used are of the simplest character.

"Under the circumstances the Commissioners regret that they are not in a position to assist your Committee.

"I have the honour to be, Sir,
"Your obedient Servant,
"J. E. LOCKHART.

"Capt. Abney, R.E."

"M. 4624.

"India Office,
"Whitehall, S.W.,
"*29th May*, 1890.

"SIR,—I am directed by the Secretary of State for India in Council to acknowledge the receipt of your letter of 20th May, and to acquaint you in reply that the tests for Colour Blindness used by the Medical Board at this Office, in the examination of candidates for the Indian Service, are Holmgren's coloured wools. First a pale grass-green skein of wool is selected, and the candidate is requested to pick out from the heap of coloured wools others of the same type of colour, irrespective of shade; if he readily selects the greens he is considered to be free from Colour Blindness, though the further tests are usually applied; if he picks out any of the confusion coloured greens, drabs, pinks, yellows, &c., he is Colour Blind.

"The second test used is a light purple or rose colour; if he matches this with blues or violets he is pronounced red blind, if with greens or greys he is pronounced green blind; if he passes the second but fails in the first test his colour sense is weak.

"The third and confirmatory test is a bright red; if he is red blind he chooses dark greens and dark browns; if green blind, bright greens and bright browns.

"I am, Sir,
"Your obedient Servant,
"O. N. NEWMARCH,
"*Maj.-Gen.*,
"*Military Secretary*.

"Capt. W. Abney, R.E.,
"Science and Art Department,
"South Kensington."

"Admiralty,
"*27th May*, 1890.

"MY LORD,—With reference to letter of the 16th inst., requesting, on behalf of a Committee appointed by the Royal Society to enquire into the subject of Colour Vision, information as to the methods employed by the Admiralty for testing Colour Blindness, I am commanded by my Lords Commissioners of the Admiralty to acquaint you that candidates who are examined for entry into

the Naval Service are required to recognise without hesitation the primary colours, as well as green, the tests employed being those of coloured flags or coloured cards held at varying distances from the candidate.

"Cases of hesitation, or suspected defective colour-perception, are tested by Holmgren's wools and samples of coloured buntings used in Her Majesty's Navy.

"A special form of apparatus, in the shape of a lamp with coloured slides, for testing colour-perception at night, is under trial.

"I am, my Lord,
"Your obedient Servant,
"Evan MacGregor.

"The Lord Rayleigh,
"Royal Society,
"Burlington House, W."

"**London, Brighton, and South Coast Railway,**
"**Secretary and General Manager's Office,**
"**London Bridge,**
"***June 26th*, 1890.**

"Tests for Colour Vision.

"Sir,—Referring to your letter of the 16th instant, relative to the tests used on this Railway for Colour Vision, I have the pleasure to hand you herewith some small samples of the different coloured glasses used for signals, and also a couple of pieces of red and green bunting, which are portions of flags used for hand signals by our Guards and Permanent-way men. The white bunting is made of the same material.

"The memorandum overleaf describes the pigments used for signal colours, and the labels on the glasses enclosed describe the colours and the uses the glasses are put to.

"With regard to the tests employed, I may state that the Company's Medical Officer examines, on their appointment to the service, the men in the Traffic Department, and for this purpose skeins of coloured wools are used, as well as coloured discs.

"In the Locomotive Department the Drivers and Firemen first commence as Engine Cleaners, when their eyesight is tested by the Inspectors, the colours shown being red, green, and white, at short distances. On being promoted to Firemen it is again tested at a distance of about 420 yards with red, green, and white boards, of about half the size of semaphore signals, and this test is again repeated when they are promoted to Drivers.

"In addition to this, the Foremen examine them at distances varying from 400 to 700 yards, both by night and day, the Surgeon of the district finally giving a certificate.

"I am, Sir,
"Your obedient Servant,
"A. SARLE,
"*Secretary and General Manager.*"

"London and North Western Railway,
"Secretary's Office, Euston Station,
"London, N.W.,
"*June 27th*, 1890.

"SIR,—I beg to acknowledge the receipt of your letter of the 16th current, and in reply to your enquiry am instructed to inform you that the Company purchase the coloured glasses used for signal lamps from—

Messrs. Chance Brothers & Co., Glass Works, Birmingham.
" Defries & Sons, 147, Houndsditch, E.C.
" Gammon & Co., Belmont Glass Works, Birmingham.

And that they obtain the material for signal flags from Messrs. W. Bancroft & Sons, Halifax.

"I am, Sir,
"Faithfully yours,
"F. HARLEY,
"*Secretary.*

"Capt. W. de W. Abney, C.B.,
"The Royal Society, Burlington House, Piccadilly, W."

"Metropolitan Railway,
"General Manager's Office,
"32, Westbourne Terrace, London, W.,
"*June 28th*, 1890.

"COLOUR VISION.

"SIR,—In response to your letter of the 16th inst., I may advise you that we have no appointed examiner to test the Colour Vision of our men, nor do we adopt the principle of colour glasses.

"Our test is that known as the wool test, adopted by several of the Railway Companies, and it is made either by our Locomotive Superintendent personally, or by his immediate representative.

"In compliance with your request, I have pleasure in sending the following samples:—

"1 red flag.
"1 green flag.
"1 piece each of red and green glass.
"1 small bottle containing vermilion enamel, with which we paint the Signal Arms.

"I am, Sir,
"Your obedient Servant,
"J. BELL.

"Captain Abney, R.E., C.B."

"South Eastern Railway,
"General Manager's Office,
"London Bridge Station, S.E.,
"*June 27th*, 1890.

"COLOUR BLINDNESS.

"SIR,—With reference to your circular of the 16th instant on this subject, I beg to state, so far as concerns the practice of this Company in this matter, in connection with those entering their service, it is as follows:—Candidates for employment as Porters, &c., are required to match colours from a collection of coloured objects or wools of various tints, and the medical man also uses the tests known as Snellen's tests.

"Applicants for employment as Engine Drivers enter the service as Engine Cleaners, and as a preliminary, a collection of coloured wools is placed before the candidate, and he is requested to pick out various colours as directed, and unless he is able to distinguish the colours readily and correctly, he is not considered eligible.

"In time an Engine Cleaner is promoted to a Fireman, and on this taking place, the colour test is again applied, supplemented with tests with hand flags at various distances.

"I am, Sir,
"Your obedient Servant,
"M. FENTON,
"*General Manager*.

"W. de W. Abney, Esq.,
"The Royal Society, Burlington House, W."

"The Great Northern Railway,
"General Manager's Office,
"King's Cross Station,
"London, N.,
"*July 15th*, 1890.

"DEAR SIR,—In reply to your letter of the 16th ult., addressed to the Secretary, on the subject of the tests applied to men admitted to the Great Northern service for Colour Blindness, I have the pleasure to enclose for your information a copy of a Report which I have called for from the Medical Officer, giving full particulars of the tests for the traffic staff.

"I also enclose copy of a Report from the Locomotive Superintendent, with reference to the tests applied to Enginemen.

"I can only add to the information contained in these, the statement that I do not know of any cases where an accident has resulted from Colour Blindness on the part of any of the Company's servants.

"The test applied to the Enginemen is a practical one, not only for colour but for distance, which is a very necessary element.

"Yours truly,
"H. OAKLEY.

"W. W. Abney,
"The Royal Society, S.W."

ENCLOSURE.

"The Great Northern Railway,
"Locomotive Department,
"Engineer's Office, Doncaster,
"*February*, 4*th*, 1890.

"DEAR SIR,—Drivers' eyesight. Yours of the 13th ult., and Mr. Clement E. Stretton's inquiry.

"When Enginemen are first appointed they are subjected to a rigid test, both with respect to distance and colours.

"For distance, the ordinary signals in the yards are used, and to ascertain their faculty for distinguishing colours a painted board is mostly employed.

"Men are again examined when age, infirmity, or any other cause leads us to suspect that their eyesight is in any way defective.

"I may tell you that in my long experience and that of my oldest assistant, no single case of Colour Blindness has occurred, and it should also be borne in mind that there are always two pairs of eyes on the footplate.

"Yours truly,
(Signed) "P. STIRLING.

"H. Oakley, Esq.,
"King's Cross."

"Belvedere House,
"Barnet,
"*June* 28*th*, 1890.

"DEAR SIR,—In reply to your letter of the 25th inst., requesting me to inform you as to the mode of testing the sight of the men. Each man is placed with his back to the light at a distance of 15 feet, and made to count the dots on a test dot card, first with both eyes and then with each separately. I also made them read the names of stations which are printed on cards at the same distance. If satisfied with the examination on this point, I then test for Colour Blindness by the use of Holmgren's coloured wools. They consist of a collection of small skeins of coloured Berlin wool, each of which is loosely twisted up. In this bundle is included wools of red, orange, yellow, yellow-green, pure green, blue-green, blue, violet-purple, pink, brown, grey, several shades of each colour. These worsteds being placed in a pile on the table, I lay aside a skein of a special colour desired for the examination, I then require the man to select from the wools other skeins which most closely resemble the colour of the sample, and to place them by its side. The Colour Sight is decided by the manner in which he performs his task. I hold up the different colours to him at a distance of 15 feet. Test 3 is a confirmatory test, and specially useful in examining the Colour Sight of those employed in reading signals. Select a

vivid red skein, like the red flag used for signals on railways, a bright yellowish-red, a scarlet. The red blind will match the sample with a dark green or dark brown with shades which to the normal eye are darker than scarlet. The green blind will select light green or light brown to match the scarlet shades which are lighter than the sample.

"Yours faithfully,
(Signed) "W. J. HARNETT.

"W. Latta, Esq."

"**Metropolitan District Railway,**
"**Manager's Office,**
"**Parliament Mansions, Victoria Street,**
"London, S.W.,
"*July* 17*th*, 1890.

"SIR,—Your letter of the 16th ultimo, addressed to the Secretary, respecting the various tests for Colour Vision, has been handed to me, and I beg to reply to your several questions as follows:—

"The tests are applied by the Company's Medical Officer, Dr. R. Bligh Wall, of 72, Bishop's Road, Bayswater. His test is by means of coloured wools, the person under examination having to name a given colour, and in some cases he is required to match colours.

"Any application to attend one of your meetings to explain the methods in detail, if addressed to Dr. Wall, will, I have no doubt, receive his best attention.

"I send herewith samples of the different coloured glass in use for the different signals, viz., red and green, as well as sample pieces of bunting used for hand signals. With respect to the fixed day signals, in the case of those known as ground discs which depend on colour, the pigment used is known as Bennett's Enamel White Paint, and Bennett's Vermilion Fluid Enamel, two specimens of which I enclose.

"I am, Sir,
"Your obedient Servant,
"ALFRED POWELL,
"*Manager.*

"Capt. W. de W. Abney,
"The Royal Society."

"**London, Chatham and Dover Railway,**
"**Secretary's Office,**
"**Victoria Station, Pimlico, S.W.,**
***July* 14*th*, 1890.**

"SIR,—In reply to your letter of the 16th ult. as to the steps taken in this Company to test the Colour Vision of the employés, I beg to inform you that the test used in our Locomotive Department is the same as that in the Army. It is a test in colours and dots on a card. I enclose a copy of the card, as well as the

description and instructions printed at the back. The card was obtained at the Horse Guards some time ago. As regards the test adopted in the Superintendent's Department for Signalmen and others under the control of the Traffic Superintendent, I cannot do better than send you a copy of a report from our Medical Officer with reference to the means which he adopts to test the Colour Vision of the men in that Department.

"Yours faithfully,

"JOHN MORGAN,

"*Secretary.*

"The Secretary, Colour Vision Committee,

"Royal Society, Piccadilly, W."

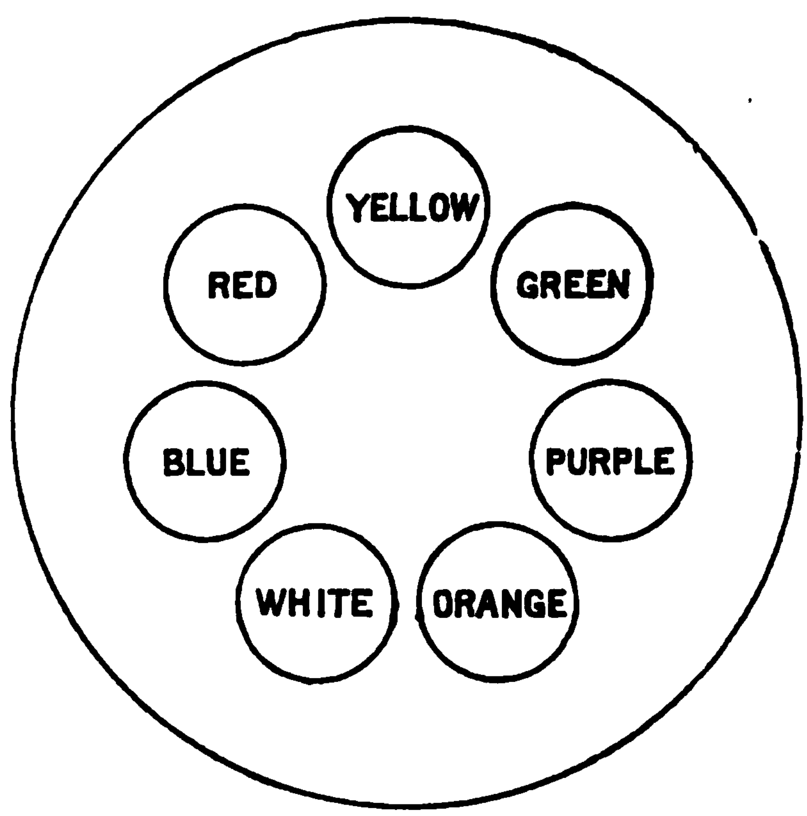
"The Avenue,

"Brixton Hill,

"*July* 9, 1890.

"Dear Sir,—In accordance with your instructions of July 5th to report on the means employed to test the railway servants on L.C. & D.R. as to their ability to detect colours, the following apparatus is in use.

"It consists of a hollow tube about 12 inches square and 22 inches long. At one end of it is a revolving disc having let into it as near as possible the seven primary colours, great care being taken that the red, green, and purple are of the same hue as the actual signals:—



"The disc is illuminated at the back, thus giving the colours much the appearance they have on the signals.

"This appliance meets with all necessary requirements, and is a fair test as to the men's capabilities of detecting colour.

"On examination they are instructed to look down the tube, and by means of a handle any one of the colours can be shown at will, so that no two men coming up need have the same series of colours. This is very important, as when a number of candi-

dates come up together, they immediately communicate to the other what has taken place.

"The actual cases of Colour Blindness are very scarce, but it is not at all an unfrequent occurrence to find men coming up, more particularly from the rural districts, quite unable to name the colours correctly, purely from want of education. These men are always rejected.

"Yours faithfully,
(Signed) "J. H. PARKER WILSON, F.R.C.S.

"To the Superintendent L.C. & D.R."

"The Cunard Steam-ship Company, Limited,
"Secretary's Office,
"Liverpool,
9th July, 1891.

"SIR,—Referring to your letter of the 23rd ult., I beg to annex particulars of the tests required to be passed by seamen before they are admitted to the Cunard service.

"I enclose also skeins of wool similar to those used on each occasion.

"The officers undergo a special examination for colour by the Board of Trade in passing each grade.

"The Cunard Company had the whole of their officers examined some two or three years ago by a qualified medical man, which examination is to be repeated this year, and every three years in future. A special examination by a medical man is also to be made in respect of each new officer entering the service.

"As a representative of the Company, if he were to attend a meeting of the Committee, could only repeat the particulars here given, my Directors think that you will probably consider such attendance unnecessary.

"I am, Sir,
"Your obedient Servant.
A. W. MONHOUSE,
"*Secretary.*

"Captain W. de W. Abney, C.B., R.E.,
"Science and Art Department,
"South Kensington, London, S.W."

COLOUR BLINDNESS.

Tests.

First.—A lamp fitted with slides in which a red, white, or green glass can be placed. When the crew are about to sign articles the lamp is lighted, and if a sailor unknown to the officers wishes to ship, if his qualifications are satisfactory, he is told to name the colour of the light as the different coloured glasses are put in the slides and shown to him.

Second.—Several skeins of coloured wool are placed on a table, and if a stranger to the officers wishes to ship, he is told to pick

out a colour named to him. Afterwards the surgeon or an officer takes up one skein after another, and asks the man to name the colour.

If the man's answers to either of the above are satisfactory, he is entered.

"Peninsular and Oriental Steam Navigation Company,
"Offices, 122, Leadenhall Street,
"London, E.C.,
"*July* 6, 1891.

"Sir,—We regret we have not been able to reply earlier to your letter of the 23rd ult., which was duly received, but we are pleased now to give you the information you desire.

"Every navigating officer who enters our service has his sight specially tested, and he is not accepted unless he possesses good normal vision in both eyes.

"The method by which the vision is tested is as follows:— One of Pickard and Curry's (of Great Portland Street) large sheets of test types is enclosed in a frame and hung on the wall of a room. The prescribed distance has been measured and marked, and the candidate is placed with his toes to this line. First one eye is covered up and he is asked to read all the lines, beginning with the very large type at the bottom line. If the eye first examined proves satisfactory, it is covered up and the other eye is examined in a similar way. Should the candidate prove to have good long sight in both eyes, his short sight is tested by his being asked to read a list of proper names printed in small type, the sheet on which the names are printed being gradually brought closer to his eyes until the words cease to be distinct. This distance is noted, and if shown to be the normal distance, his sight is considered good. We attach importance to good short sight as well as good long sight on account of the necessity of reading, marking on charts, &c.

"The candidate having shown himself to possess good long and good short sight, is tested as to colour-sight by being asked to name the colours of an assortment of the usual coloured wools, obtained for the purpose from Pickard and Curry.

"Preliminary to this examination, the candidate's power of long sight is usually roughly tested by his being asked to read letters on sign-boards, &c. at various distances in the street, but it is on the accurate tests above described, which are never omitted, that we place reliance, and on the results which they give we base our decision regarding the acceptance of the candidate so far as his vision is concerned. All seamen have to produce Board of Trade Colour Certificates before being shipped by this Company's vessels.

"We are, dear Sir,
"Yours faithfully,
"J. D. JAMES,
"*For the Managing Directors.*

"Captain W. de W. Abney, C.B., R.E.,
"Science and Art Department."

APPENDICES.

Appendix I.

Statistics of Colour-blindness.

The following schools and institutions were examined by the Committee of the Ophthalmological Society of London:—

Westminster School.
Eton.
King's College School.
University College School.
Christ's Hospital (Blue Coat School).
Merchant Taylors' School.
Friends' School, Saffron Walden.
" " Scarborough.
" " York.
" " Ackworth.
" " Didcot.
Ley's School, Cambridge.
Royal Medical Benevolent College, Epsom.
City of London School.
Jews' School, Bell Lane.
Jews' School, Greek Street, Soho.
Duke of York's School.
Foundling Hospital.
Haverstock Orphan Asylum.
Hanwell Lunatic Asylum.
Fulbourne Lunatic Asylum.
Deaf and Dumb Schools, Kent Road.
Metropolitan Police.
Royal Naval School, Greenwich
St. Thomas's Hospital Medical School.
Coldstream Guards.
Beddington Orphan Asylum.
Various Schools in Dublin.

It will be observed that some of the foregoing institutions would supply subjects derived from special classes of persons, such as Jews and deaf-mutes; while others would be fairly representative of the whole community. The examinations were conducted by Holmgren's method, supplemented, in some instances, by the use of coloured lights, and the examiners, sixteen in number, were all of them surgeons engaged in ophthalmic practice. The Committee introduced their Report by the following prefatory observations:—

"Your Committee becomes more and more convinced that a competent examiner is not made in a day, or even in a month, and that, even with large experience, much judgment and capacity are needful to interpret rightly the acts of the examined. This necessity is perhaps most strongly exhibited in the case of intelligent persons who are incompletely colour-blind. Such persons, though they may have a much feebler appreciation of the difference between red and green, for example, than is normal, may, after accurate observation and comparison, separate the red skeins of wool from the green. When tested, however, at various distances with coloured lights, their defects are strikingly apparent, and it becomes clear that they are totally unfitted for responsible posts in which rapid appreciation of colour at a distance is required.

"Colour-blindness is here taken as implying a defective recognition of the difference between colours. No account is taken of

incapacity to distinguish between different shades of one colour, or even of an inability to distinguish between such colours as blue and violet or blue and green, when these are the sole defects. It is perfectly and clearly distinguished from a defective naming of colours, and from deficient acuteness of vision."

The actual results of examination are stated as follows:—

"The total number of persons examined was 18,088, of whom 16,431 were males and 1,657 females.

"The examination of certain classes of persons was undertaken in the expectation of some peculiarities, which the result amply justified.

"Deducting these, we have 14,846 males, of whom 617 were colour-blind, giving an average percentage of 4·16. Making similar deductions in the case of the females, we have 489 persons, with 0·4 per cent. And even this small percentage is entirely made up of persons with very slight individual defects.

"Taking the exceptional groups of females, we find those of Jewish extraction, the members of the Society of Friends, and the inmates of deaf and dumb asylums, to be more defective as regards colour than the average. Thus, among the first (730) examined as high a percentage as 3·1 was touched, though the cases were almost entirely of slight character. Among the members of the second group who happened to be the subjects of examination (216 in all) the percentage was even somewhat higher (5·5), though the cases were clearly even slighter still. Among the deaf and dumb females (122 examined) there was a somewhat high percentage (2·4) of slight cases.

"Colour defects exceeded the average in the male members also of the same classes. It is possible to draw more exact comparisons between the normal and colour-blind males than of females, because the former show much more pronounced cases. Thus, the slight individual differences of examiners will cease to be a source of error for males, for no examiners, however low the standard they exact, could omit to detect and record as colour-blind those persons who matched red, or the full shades of brown or grey, with green.

"Enumerating in this manner, we have—

"Among males generally (14,846 examined) 3·5 per cent. of pronounced cases.

"Among male Jews (949 examined) 4·9 ditto, ditto.

"Among male Friends (491 examined) 5·9 ditto, ditto.

"Among male deaf and dumb (145 examined) 13·7 ditto, ditto.

"It must be noted, however, that the Jews were, on the average, in a poorer condition of life than any other class examined. The deaf-mutes were mostly poor. The Friends were mostly of the middle class; their mistakes were chiefly confined to the paler shades, and were therefore, in general, slight in degree, especially as compared with the Jews, whose defects, though less numerically, were usually well pronounced in character. The wealthier Friends are much less liable to

colour-defects than their poor; but even among them the males exceed the average.

"There are naturally difficulties attending the examination of the deaf-mutes. But after repeated examinations had made the process perfectly clear to them, it was apparent, from the nature of their mistakes, that there was among them a very high average of colour-defects. Those examined inhabit schools in London, and naturally live in a condition of considerable isolation from the surrounding world.

"It is worthy of note that, when in any class of persons colour-defects exceed the average in number and intensity, there is often an unduly high proportion of red-blind as compared with green-blind. This is especially marked among the Jews, for among them the pronounced cases of red-blindness were 3·6 per cent. of the whole number examined, against 1·3 per cent. of green-blindness; whereas among males generally the red-blind were 2 per cent., and the green-blind 1·5 per cent.

"A large number of male children (2,859) were examined in Dublin by Mr. Swanzy. Their average is somewhat higher than that found in England, being 4·2 per cent. of pronounced cases. But as the examinations were necessarily made by different hands, and as the boys examined in Dublin were, on the average, of poorer class than those furnishing the corresponding statistics in this country, we must be cautious in inferring a greater average percentage of colour-defects in Ireland than in England.

"Interesting results are derived from a comparison of the percentages in the different groups examined, especially with regard to their different positions in the social scale, by which is implied presumably a corresponding difference in education.

"Thus, in England, among the police (4,932 examined), and in schools of about the same social rank (1,729 examined), the pronounced cases form 3·7 per cent. In middle-class schools the same form 3·5 per cent. In the professional class, as represented by medical students and sons of medical men (435 examined), the same form 2·5 per cent. Among the boys at Eton the same form 2·46 per cent.

"And even more striking instances are recorded in Ireland, by Swanzy, who finds the percentage among the sons of artisans and labourers (2,486 examined), to be nearly twice as great as among the sons of the professional and wealthier classes.

"Nor can any observer fail to be struck by the much greater certainty and rapidity with which the children of the upper classes pick out the various shades of the same colour. And the momentary confusion of blue and green, which is not uncommon among the poorer classes, independently of any defect of colour-vision, is very rarely seen among the others."

The following report regarding the tests of a Japanese regiment was communicated by Mr. Brudenell Carter:—

Table of the Results of Investigations on Colour-blindness.

First experiment made on the 20th (clear-weather) September, the 17th year of Meiji (1884), from 9.5 a.m. to 5 p.m.

Number of persons examined—
600 soldiers of the First Regiment of Infantry of the Tokio garrison.

Place of experiment—
A room in the Hospital of the First Regiment of Infantry.

Examiner—
Medical Officer of a Swedish man-of-war.

Assistants—
Taniguchi Ken, 2nd-class Surgeon of the Imperial Army; Ume Kinnojo, in the service of the Tokio University.

Mode of experiment—
Trials with woollen yarns.

Results.

Red colour-blind	12
Green „	4
Incomplete „	5
Weak colour-vision	15
Total	36

Second experiment made on the 22nd (clear weather) September, the 17th year of Meiji (1884) from 8.30 a.m. to 4 p.m.

Number of persons examined—
600 soldiers of the Third Regiment of Infantry of the Tokio garrison.

Place of experiment—
A room of the Hospital of the Third Regiment of Infantry.

Examiners and mode of experiment—
The same as in the previous experiment.

Results.

Red colour-blind	7
Green „	6
Incomplete „	7
Weak colour-vision	12
Total	32

Appendix II.

Board of Trade Tests.

The following is the Circular of the Board of Trade relating to their colour tests. The luminosities and the dominant wave lengths have been shown in *italics* opposite the colours used, which will give an idea to the Committee of the utility of the colours employed, recollecting that the neutral point in the spectrum for the green colour-blind is about λ5020, and for the red about λ4960 :—

"Examination in Colours.

"Herewith are—

"(*a.*) A lanthorn having in it a lamp in which kerosine is to be burnt..

"(*b.*) A slide having ground glass in it.

"(*c.*) Nine slides, each having a coloured glass in it. The colours are as follow :—

	Luminosity in gaslight, white 100.	Dominant wave length.
1. Red (Standard)	*11·2*	*6,200*
2. Pink or salmon	*42·5*	—
3. Green (Standard or No. 1)..	*10·0*	*5,190*
4. Green (Bottle or No. 2) ..	*5·7*	*5,720*
5. Green* (Pale or No. 3) ..	*20·0*	—
6. Yellow	*80·0*	—
7. Neutral*	*7·5*	—
8. Blue (Standard)	*2·5*	*4,650*
9. Blue* (Pale).. .. .	*7·5*	—
Ground flues used	*58·0*	—

"(*d.*) Cards, five of each, as follows :—

1. White	*100*	—
2. Black..	*40*	—
3. Red	*140*	*6,150*
4. Pink*	*21*	*6,630*
5. Green	*240*	*5,370*
6. Drab*	*16·5*	*5,770*
7. Blue	*7·5*	*4,750*
8. Yellow	*8·0*	*5,620*

"EXAMINATION BY DAYLIGHT (CARDS).

"In conducting the examination by daylight the examiner should do it in three ways: —

"1. The cards should be mixed up. The examiner should then hold up each card separately, and ask the candidate to name the colour; and if the candidate does so without hesitation, he is to be regarded as having passed the daylight test.

"2. If the candidate hesitates in any of his answers so as to raise a doubt in the mind of the examiner as to his ability to readily distinguish colours, the examiner should put all the cards on the table and require the candidate to select all cards of a colour or colours named by the examiner.

"3. Having done that, they should all be mixed up again, and the candidate should be required to sort the cards into eight heaps, putting all of one colour into each heap.

"4. The result of the examination should be noted and recorded in each case.

"EXAMINATION BY ARTIFICIAL LIGHT.

"The room should be dark.

"The lamp lighted and placed in the lanthorn.

"The applicant should be seated or should stand so as to be opposite to the opening of the lanthorn; and, at least, 15 feet from the front of the lanthorn.

"He should first of all see the light in the lanthorn without the interposition of any glass, and be asked if it appears to him to have any colour, and if so what colour?

"The slide with the ground glass should then be put into the opening at the front of the lanthorn which is nearest to the light, and the applicant asked the same question.

"The slide with the ground glass is to be left in, and the slides with the coloured glasses placed one by one, and separately, in front of it, and the candidate asked in each case to name the colour or tint.

"The result of the examination should of course be noted and recorded in each case.

"GENERAL.

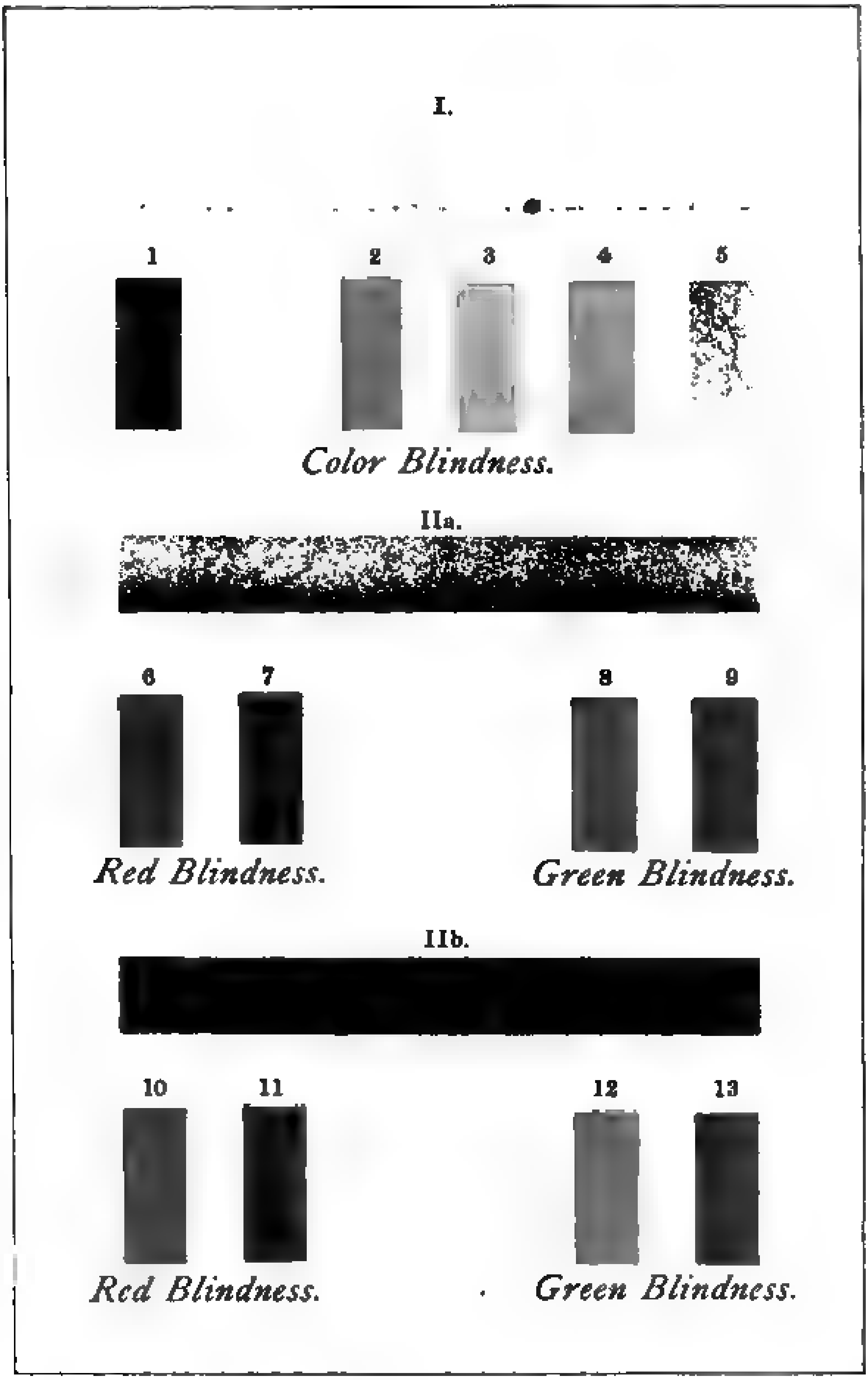
"The cards and glasses against which a star is placed in the list are what may be called confusion tints. The candidate is not to be regarded as having 'failed' if he miscalls these tints, provided that he names all the others correctly. But if, having named all the others correctly, he miscalls these so far as to name the drab card, No. 6, as red, pink,

blues, violets, purples, pinks, browns, and greys. Several shades of each colour, with at least five gradations of each tint, should be procured, from the deepest to the lightest greens and greys. Varieties of pinks, blues, and violets, and of light grey, together with shades of brown, yellow, red, and pink, must be especially well represented. The test skeins with which the examinees are to compare the colours should be three in number: a light green, a pale purple or pink, and a bright red. These three colours will suffice to indicate approximately the amount and kind of colour-blindness which may exist. The light green skein, which is a tolerably pure green mixed with a large proportion of white, is chosen as the colour which closely matches the spectrum colour which the red- and green-blind distinguish as white or grey. It is chosen of a pale tint, as it then becomes puzzling to the colour-blind to distinguish its colour by its luminosity. A light grey or drab skein will present the same brightness to him that this pale colour does, and although he may be trained to distinguish bright colours by their relative luminosities, in the case of these pale varieties he will be unable to do so. The light purple or pink is chosen for similar reasons, and in fact it is nearly a complementary colour to the green. The purple is, according to the Young-Helmholtz theory, a mixture of two fundamental colours, the blue and the red, and as in the green-blind it excites both the blue and red sensations it may be confused with grey, or with a green. In the red colour-blind it excites in excess the blue sensation mixed with what they call white. A blue or violet may therefore be matched with it.

The method of examination is as follows:—

"*Method of Examination and Diagnosis.*

"The Berlin wools are placed in a heap on a large table, covered by a white cloth, and in broad daylight. A skein of the test-colour is taken from the pile, and laid far enough away from the others not to be confounded with them during the examination. The person examined is requested to select other skeins from the pile most nearly resembling it in colour, and to place them by the side of the sample. At the outset, it is necessary that he should thoroughly understand that he is required to search the heap for the skeins which make an impression on his chromatic sense, and quite independently of any name he may give the colour, similar to that made by the test-skein. The examiner should explain that resemblance in every respect is not necessary; that there are no two specimens exactly alike; that the only question is the resemblance of the *colour*; and that, consequently, he must endeavour to find something similar in shade, something lighter and darker of the same colour, &c. If the person examined cannot succeed in understanding this by a verbal explanation, resort must be had to action. The examiner should himself pick out the skeins, thereby showing in a practical manner what is meant by a shade,



W GRIGGS LITH

and then restore the whole to the pile, except the sample-skein. As it would require too much time to examine every individual in this way, it is advisable, when examining large numbers, to instruct them all at once, and to ask them to attentively observe the examination of those preceding them, so as to become more familiar themselves with the process. This saves time and there is no loss of security, for no one with a defective chromatic sense will be able to find the correct skeins in the pile the more easily from having a moment before seen others looking for and arranging them. He will make the same characteristic mistakes; but the normal observer, on the other hand, will generally accomplish his task much better and more quickly after having seen how it has to be done.

"The coloured plate (*see* Plate II) is for the purpose of assisting the examiner in the choice of his colours, and to help him to decide the character of the colour-blindness from the mistakes made. The colours in the plates are of two characters:—

"1st. The *colours for samples* (*test-colours*); that is, those which the examiner presents to the persons examined; and

"2nd. The '*confusion colours;*' that is to say, those which the colour-blind will select as matches with the sample.

"The first are shown on the plate as horizontal bands, and are distinguished by Roman numerals; the second as vertical bands, under the test-colours, and are distinguished by Arabic figures.

"The coloured table is not intended to be used as a test; it is simply to assist the examiner in his choice of correct test-colours, and to help him to diagnose the special form of colour-blindness.

"As to the similarity between the confusion-colours of the plate, and the wools which the colour-blind take from the heap, reliance must be placed simply on the hue, and not on their brightness or degree of colour saturation. In all cases where we have to vary from this rule we must hold to the relative rather than the absolute saturation. The confusion-colours shown in the plate are only to illustrate the mistakes which the colour-blind will make, and for this purpose they serve perfectly. Having made this explanation, we can pass directly to the test itself. The following are the directions for conducting it, and for making a diagnosis from the results:—

"TEST I.—The *green* test-skein is presented. This sample should be the palest shade (the lightest) of very pure green, which is neither a yellow-green nor a blue-green to the normal eye, but fairly intermediate between the two, or at least not verging upon yellowish-green.

"*Rule.*—The examination must continue until the examinee has placed near the test-skein all the other skeins of the same colour, or else, with these or separately, one or more skeins of the class of 'confusion colours' (1–5), or until he has sufficiently proved by his manner that he can easily and unerringly distinguish the confusion colours, or given unmistakable proof of a difficulty in accomplishing it.

"*Diagnosis.*—An examinee who places with the test-skein 'confusion colours' (1–5)—that is to say, finds that it resembles the 'test-colour'—is *colour-blind*, whilst if he evinces a manifest disposition to do so, though he does not absolutely do so, he has a *feeble chromatic sense.*

"*Remark.*—We might have taken more than five colours for 'confusion;' but we must remember that we are not taking into consideration *every* kind of defective colour-sense, but only those which are important in connection with railways.

"As to No. 1, which represents a grey, we would remark that too much stress must not be laid on its luminosity, or on any slight difference in its hue from the grey skeins which the examinee puts with the sample.

"If it is only required to determine whether a person is colour-blind or not, no further test is necessary, but if we want to know the kind and degree of his colour-blindness, then we must proceed with the next test.

"TEST II.—A purple skein is shown to the examinee. The colour should be midway between the lightest and darkest. It will only approach that given in II of Plate II, as the colour of the wool is much more brilliant and saturated, and bluer.

"*Rule.*—The trial must be continued until the examinee has placed all or the greater part of the skeins of the same shade near the sample, or else, simultaneously or separately, one or more skeins of 'the confusion-colours' (6–9). If he confuses the colours he will select either the light or deep shades of blue and violet, especially the deep (6 and 7), or the light or deep shades of one kind of green or grey inclining to blue (8 and 9).

"*Diagnosis.*—1. A person who is proved colour-blind by the first test, and who, in the second test, selects only purple skeins, is *incompletely colour-blind.*

"2. If, in the second test, he selects with the purples blue and violet, or one of them, he is *completely red-blind.*

"3. If, in the second test, he selects with purple only green and grey, or one of them, he is *completely green blind.*

"*Remark.*—The red-blind never selects the colours taken by the green-blind, and *vice versâ.* The green-blind will often place a violet or blue skein by the side of the green, but it will then only be the brightest shades of these colours. This does not affect the diagnosis.

"The fact that, in this test, many green-blind select, besides grey and green or one of these colours, also bright blue, has led to misunderstanding. Some have concluded from this that red and green blindness may exist together in the same individual; others have thought that these two kinds of colour-blindness are not readily distinguished by this method. The first conclusion is not correct. The two kinds of colour-blindness have great similarity, but differ in innumerable slight variations. They are to be considered as two sharply defined classes.

"The second conclusion can only arise from not understanding

and not using the method correctly. The especial purpose of this method must be kept constantly in view, viz., to find the characteristics of the defects in colour-perception of those examined. The characteristic of green-blindness is the confusion of purple with grey or green, or both. This confusion is the point to be determined: everything else may be neglected. A complete colour-blind, who confuses purple with grey or green (bluish-green), or with both, is *green-blind, do what else he may.* This is the rule, and the careful and observant examiner who understands the application of the test, will at once distinguish it. It is, indeed, often possible, in marked cases of incomplete colour-blindness, to decide to which class it belongs to by the way the examined acts with his hands. We do not mean by this that the diagnosis is always very easy. Practice and knowledge are necessary. As there is a long series of degrees of incomplete colour-blindness between normal vision on the one hand, and complete colour-blindness on the other, there must naturally be a border line where differences of the two kinds of colour-blindness cease to be recognised.

"The examination may end with this test, and the diagnosis be considered as perfectly settled. It is not even necessary, practically, to decide whether the colour-blindness is red or green. But to more thoroughly convince railway employés and others, who are not specialists, of the reality of the colour-blindness, the examination may be completed by one more test. It is not necessary to the diagnosis, and only serves as a confirmation.

"Test III.—The *red* skein is presented to the examinee. It is necessary to have a vivid red colour, like the red flag used as signals on railways. The colour should be that of II*b* of the plate, rather towards yellowish-red.

"*Rule.*—This test, which is applied only to those completely colour-blind, should be continued until the person examined has placed beside the test skein all the skeins belonging to this hue or the greater part, or else one or more 'confusion colours' (10-13). The red-blind chooses, besides the red, green and shades of brown, which (10-11), to the normal sense, seem darker than red. On the other hand, the green-blind selects shades of these colours, which appear lighter than red (12-13).

"*Remark.*—Every case of comparatively complete colour-blindness does not always make the precise mistakes we have just mentioned. These exceptions are either instances of persons who are not quite completely colour-blind, or of completely colour-blind persons who have been practised in the colours of signals, and who endeavour not to be discovered. They usually confound at least green and brown; but even this does not always happen.

"*Mono-chromatic Vision.*—The absence of all except one colour sensation, will be recognised by the confusion of every hue having the same intensity of light.

"*Violet-blindness* will be recognised by a genuine confusion of purple, red, and orange in the second test. The diagnosis should

be made with discrimination. The first test often shows blue to be a 'confusion colour.' This may, in certain cases, be the sign of violet-blindness, but not always. We have not thought it advisable to recognise defects of this kind; and only the marked cases, that other tests establish as violet colour-blindness, should be reckoned in the statistics."

Dr. Joy Jeffries, in his book on colour-blindness, gives a translation of Holmgren's special directions for conducting the examinations:—

"*Special Directions for Conducting the Test.*

"The method, as we have said, plays an important part in an examination of this kind, not only from the principles upon which it rests, but also from the manner in which it is used. The best plan for directing how to proceed is by oral instructions and *de visu*; but here we are obliged to accomplish this by description. Now, this is always defective in some respects, especially if we wish to be brief. What has been said would evidently suffice for an intelligent and experienced physician; but it may not be superfluous to enter still further into detail to provide against any possible difficulties and loss of time. The object of the examination is to discover the nature of a person's chromatic sense. Now, as the fate of the one to be examined and that of others depend upon the correctness of the judgment pronounced by the examiner, and that this judgment should be based upon the manner in which the one examined stands the trial, it is of importance that this trial should be truly what it ought to be,—a trial of the nature of the chromatic sense, and nothing else,—an end that will be gained if our directions are strictly followed. It is not only necessary that the examiner should carefully observe them—which does not seem to us difficult—but that he also should take care that the individual examined does thoroughly what is required of him. This is not always as easy as one might suppose. If it were only required to examine intelligent people, familiar with practical occupations and especially with colours, and with no other interest connected with the issue of the examination than to know whether they are colour-blind or not, the examination would be uniform and mechanical; but it is required to examine people of various degrees of culture. all of whom, besides, have a personal interest in the issue of the examination. Different people act very differently during the examination for many reasons. Some submit to it without the least suspicion of their defect; others are convinced that they possess a normal sense. A few only have a consciousness, or at least some suspicion, of their defect. These last can often be recognised before examination. They will keep behind the others, and attentively follow the progress of the trial; and, if allowed, will willingly remain to the last. Some are quick; others, slow. The former approach unconcernedly and boldly; the latter, with over-anxiety and a certain dread. Some have been

perhaps already tested, and practised themselves in preparation for the trial; others have never been familiar with colours. Among those already tested some may be colour-blind. Some of these latter are uncertain about their mistakes, and act with great care; whilst others again, having been practised in distinguishing signals, conclude that their colour-sense is perfect. They make the trial quickly and without thought; of course regularly making the mistakes characteristic of their special form of colour-blindness.

"The majority, however, desire to perform their task as well as possible; that is, to do what the normal-eyed does. This of course assists in testing them, provided it does not lead to too great care, as then the testing the colour-blind is more difficult; the trouble being that much time is thus wasted. Only a very small part have a contrary desire; namely, to pass for colour-blind, though normal-eyed. We will speak of these later, and now only concern ourselves with those who stand the test in good faith with the desire to appear normal, though perhaps they are colour-blind.

"The trial generally goes on rapidly and regularly. We will only mention those hindrances and peculiarities which most frequently occur. The examiner must watch that no mistake is made from not understanding. The names of the colour need never be used, except to ascertain if the name learned hides the subjective colour-sensation, or to find the relation between the name the colour-blind employs and his colour-perception.

"The person examined who thinks more of names than the test itself (this being generally a sign of school-learning) selects not only the wools of the same shades—that is, those of the same colour to his eye—but all which generally have the name of this colour: for instance, in the first test I, not only the green like the sample, but all that are green; and with the second test, not only the purple (and what are generally called red), but all which *look* reddish, scarlet, cinnabar, or sealing-wax red. This is of no importance; for those who only do this have scarcely such defective chromatic sense as that with which we are concerned. He is either normal-eyed or violet-blind. Simply as a test of violet-blindness in the interest of science, we can go on with the examination, and ascertain how far the grouping of the two colours was due to a confusion of names or to defective colour-perception. Otherwise this examination does not concern the practical point we aim at.

"Under any circumstance it is better to correct the mistakes just mentioned, when arising from misunderstanding, and it is even necessary, in reference to the mistakes we explained might occur with the first test. It might be said that it was sufficient if the examined confounded the test-colour with green only; that it was indifferent whether he distinguishes carefully between the various kinds of green. But, in fact, this is not so unimportant. We must give full weight as to whether the infraction of the

rules arises from misunderstanding, or lack of practice with colours, or, finally, from a true chromatic defect. To include all that is green would render the test tedious and unpractical. In fact, no little judgment has been exercised in the selection of the very lightest shade of the green proposed as a test-colour; for it is exactly what the colour-blind most readily confounds with the colours (1–5) of the plate. If the examinee were allowed to depart from the narrow limits established by the trial, it would include every shade of green; the result of which would be that he would prefer to select all the vivid shades, and thus avoid the dangerous ground where his defect would certainly be discovered. This is why it is necessary to oblige him to keep within certain limits, confining him to pure green specimens, and, for greater security, to recommend him to select especially the lightest shades; for, if he keeps to the darker shades, as many try to do, he readily passes to other tones, and loses himself on foreign ground, to the great loss of time and of certainty of the test. What we have just said of green applies also, of course, to purple.

"The principle of our method is to force the examinee to reveal, by an act of his own, the nature of his chromatic sense. Now, as this act must be kept within certain limits, it is evident that the examiner must direct him to some extent. This may present, in certain cases, some difficulty, as he will not always be guided, and does either too much or too little. In both cases the examiner should use his influence, in order to save time and gain certainty; and this is usually very easily done. This intervention is of course intended to put the examinee in the true path, and is accomplished in many ways, according to the case in point.

"We will here mention some of the expedients we have found useful:—

"(A) *Interfering when the Examined select too many Colours.*

"It is not always easy to confine the one examined within the limits of the method. He easily slips amongst the sorted colours for the first test, for example, a yellow-green or blue-green skein among the others, and, as soon as there is *one*, others follow usually; and it thus happens that in a few moments he has a whole handful of yellow-green, a second of blue-green, a third of both these shades at the same time. Our procedure has assisted us in more than one case of this kind.

"(*a*) When the person examined has begun to select shades of one or several other colours than those of the sample, his ardour is arrested by taking from him the handful of skeins he has collected, and asking him whether his eye does not tell him there are one or several which do not match the others, in which case he is solicited to restore them to the pile. He then generally remarks that there is some obscuration, and proceeds in one of the following manners:—

"1. He rejects, one after the other, the foreign shades, so

that the correct remain, which is often only the sample-skein. He is shown what mistake he has made. Names are used to remind him that one class of green may be yellow-green; and another, blue-green; and, to induce him to avoid them, he is advised only to select skeins of the same shade as the specimen, although they be lighter or darker, and have neither more yellow nor blue than that. If his first error arose only from a misconception or want of practice in handling colours, he begins generally to understand what he has to do, and to do properly what is required of him.

"2. Or else he selects and rejects immediately the skein of the sample itself. This proves that he sees the difference of colour. He is then shown the skein as the only correct one, and asked to repeat the trial in a more correct manner. He is again put on the right track as just before; and the trial proceeds rightly, unless the error arose from a defect in the chromatic sense. Many seem, however, to experience a natural difficulty in distinguishing between yellow-green and blue-green, or the dull shades of green and blue. This difficulty is, however, more apparent than real, and is corrected usually by direct comparison. If the method requiring the name of the colour to be given is used, a number of mistakes may be the result. If a skein of light green and light blue alone are presented to him, asking him to name them, he will often call blue green, and green blue. But if, in the first case, a blue skein is immediately shown him, he corrects his mistake by saying 'this is blue,' and 'that green.' In the last case it happens so *mutatis mutandis*. This is not the place for an explanation. It must suffice to say that the error is corrected by a direct comparison between the two colours.

"There is, according to the theory, one class of the colour-blind—violet-blind—who, in consequence of the nature of their chromatic sense, and, therefore, notwithstanding the comparison, cannot distinguish blue and green. But our method has nothing to do with this class of the colour-blind, because such are not dangerous on railways.

"(*b*) *Another Process.*—If the one examined place by the side of the sample a shade, for instance, of yellow green, the examiner places near this another shade, in which there is more yellow, or even a pure yellow, remarking, at the same time, that, if the first suit, the last must also. The other usually dissents from this. He is then shown, by selecting and classing the intermediate shades, that there is a gradation, which will diverge widely if logically carried out as he has begun. The same course is followed with colours of the blue shades, if the blue-green were first selected. He sees the successive gradations. and goes through with this test perfectly if his chromatic sense is correct.

"To ascertain further whether he notices these additions, or the tints of yellow and blue in the green, we can ourselves take the yellow-green and blue-green to ask him if he finds this to be

so. We can judge by his answer of his sense with regard to these shades, and the object of this investigation is accomplished.

"It results from all this that many who are finally considered to have a normal chromatic sense may occasionally cause embarrassment. In the main, the normal observer of this kind causes greater loss of time than the colour-blind. It is astonishing to see with what rapidity the colour-blind betray their defect. At least it is found, in the majority of the cases examined by us, that the first skein of wool selected from the pile by the colour-blind in the first test was one of the 'colours of confusion.'

"(B) *Interfering when the Examined select too few Wools.*

"Those who evince too great slowness also require the interferences of the examiner in another manner. We can lay aside here those cases in which, at the sight of the complex colours of the heap of wool, the examined finds it difficult to select a skein resembling the sample in a collection where all the particular colours seem to differ from each other, and in consequence declares immediately that he can find none resembling the specimen. It is replied that an absolute resemblance is not demanded, and that no one asks impossibilities; that time is limited, many are waiting, &c. But there are people who—from natural slowness, from being unaccustomed to such business, from fear of making mistakes, especially if they have been previously examined and been suspected of colour-blindness, or from many other motives—proceed with the greatest caution. They do not even wish to touch the wool; or they search, select, and replace with the greatest care all the possible skeins without finding one corresponding with the sample, or that they wish to place beside it. Here, then, are two cases: on one hand, too much action with the fingers, without result; on the other, too little effort. The examiner is forced to interfere in both cases.

"(*a*) At the time of a too great manual action, without corresponding practical result, the examiner must be careful that the eye and hand act simultaneously for the accomplishment of the desired end.

"Some people forget that the hands should be subservient to the eye in this trial, and not act independently. Thus they are often seen to fix their eyes on one side while their hands are engaged on the other. This should be corrected, so as to save time and avoid further labour. When, from the manual activity of the one examined, or by the unobserved aid of the examiner, all the correct skeins, or only a portion, are found in the pile, it is wise to stop, and invite the former to cross his hands behind his back, to step back a pace, and quietly consider all the skeins, and, as soon as his eye has met one of those for which he is looking, to extend his hand and take it. The best plan is to advise him to look first at the sample, and then at the pile, and to repeat this manœuvre until his eyes find what he is looking for.

"This stratagem generally succeeds when nervousness from over-anxiety causes his hands to tremble; but it is not always easy to induce him to keep his hands behind his back until the moment for taking the skein in question.

"(*b*) In cases of great caution, the trial is hastened, if the examiner come to the assistance of the other, by holding above the pile one skein after the other, and requesting him to say whether it resembles the colour of the sample or not. It will be advisable first to select the skeins that a colour-blind person would approve. If he is so, he will approve of the selection, and the question is settled; if not, he rejects them, not without a characteristic smile, or with an expression of wounded dignity. This also enlightens us as to his chromatic sense. But even the colour-blind may, in such a case, refuse what is presented, especially if his caution is premeditated, and he suspects that a snare is intended. It is found quite frequently that he rejects the correct shades likewise presented with the others. This is not the case when one, having a normal chromatic sense, is slow and deliberative when subjected to the test under this form. He has an eye alive to the correct colours.

"One process, in cases of this last kind, is to select false samples, which are placed close to the correct one, by the side, above, or below, to attract the attention of the examined from the right side. It is necessary so to proceed that the true sample be displaced when the others are drawn out, so that the person examined may see it move. It does not, however, always happen to catch his eye. The best means is then to make him examine the whole, with his hands behind his back, and invite him to freely make his choice. But, whatever the process, it is necessary, in every case where one has been assisted in selecting a certain number of skeins which he has found analogous to the sample-colour, to make a rule not to conclude the trial without examining into the effect of the aid accorded. It is necessary to hold in the hand the approved package, and ask if he is satisfied, or if he would desire any change. If he approve the choice, the diagnosis is established. The same course must be pursued with the defective chromatic sense, that the trial may be made with or without assistance. To be thorough, the name given by the colour-blind to the colours in question may be likewise asked.

"In cases where any one suspected of colour-blindness has remained some time to see the trial of others, and where, as often happens, he has remarked the samples belonging to a required green shade, he may of course profit by it in his own trial. But this can be prevented by furtively concealing one or two of these samples. If he seem to be disposed to confound green and grey, it will be very easy to entrap him. If we do not succeed, even when assisting him, in entrapping him in this snare, the hidden samples may be put back into their places, to be convinced that the trial is correct.

"From the above, it is seen that many artifices may be

necessary in our examination. It may be regarded as an advantage of our method that it has at command a great variety of resources. We have by no means mentioned all; and yet many who have only read this description will probably reproach us with having devoted ourselves too much to details which seem to them puerile. But we believe that those who have examined the chromatic sense of a great number of persons, and acquired thereby considerable experience, will think differently.

"We are convinced that time is saved by such artifices, and a more certain result obtained; whilst a practised surgeon, who has become to a certain degree a *virtuoso*, will accomplish his object quicker and surer by such artifices than one who neglects them. Recent experience fully confirms this. All those who have familiarised themselves with my method, and have had experience with colour-blindness, and of whose competence there can be no doubt, report, without exception, that it is to be fully depended on—the most practical and the best.

"An advantage of the method was shown to be that those who were to be examined could be present and see each individual tested, without this interfering in the least with the certainty of the result. The individual test is even hastened thereby. The colour-blind, and even the normal-eyed who are not familiar with colours, are generally rather shy about being tested, in whatever way it is done. As the method, however, is carried out, they have more confidence. The majority are even amused. The old adage holds true here, that it is easier to find fault than to do it yourself. The surgeon, who watches not only the examined, but also those around, can often see from their faces how closely the latter observe the person being tested when he takes out the wrong colours, as also when he neglects the right ones under his eye. This gives those looking on confidence and assurance, till their turn comes, when they appear as uncertain as before they were confident. There is something attractive in the process, stimulating the interest, and hence is not without benefit.

"From this we see that our judgment of a person's colour-sense is made, not only by the material result of the examination—the character of the wools selected—but often also by the way the examined acts during the test. We should mention a very common manner of persons on trial, which, in many cases, is of great value in diagnosis. Often, in searching for the right colour, they suddenly seize a skein to lay it with the sample; but then notice it does not correspond, and put it back in the heap. This is very characteristic; and, if an examiner has often seen it, he can readily recognise and be assured that it is an expression of difficulty in distinguishing the differences in the colours. We frequently see this in the first test, with shades of greenish-blue and bluish-green. Here it means nothing important; but it is quite the reverse, however, when it concerns the grey or one of the confusion-colours (1-5). Uncertainty and hesitation as to these colours, which the colour-blind do not distinguish from the test

colour, even when directly comparing them, is positive proof of mistake, implying defective chromatic vision of the complete colour-blind type. No doubt the form of chromatic defect which we have called *incomplete* colour-blindness exists in several kinds and degrees. This is not the place to further discuss our experience on this point; and, for the practical purpose we have in view, it is not necessary. As we have explained, there are, among this class, forms gradually approaching normal colour-sense. How they are distinguished has been described. We designated them as possessing *feeble colour-sense.*

"It is, perhaps, not easy to detect this special form by any other method, or even by our own; we therefore give the following as a means of so doing. The only way of getting at it is by determining at what distance the examined can distinguish a small coloured surface. We have to do, in fact, with a feeble colour-sense, which does not *prevent* the colours from being distinguished, but only renders it difficult. We may suppose, in comparison to the normal- that the *feeble* colour-sense is due either to a weaker response to the stimulation of the colour-perceptive organs of the retina, or else to a stimulation of a relatively smaller number of these organs. In either case this method would give us the same result, judging from our experience in testing the eccentric portions of the field of vision with the perimeter.

"The method we here speak of shows us also the effect of habit and practice on the colour-perception, and it is worth while to dwell on this point. It not unfrequently happens that a person who by test No. I has been noted 'incomplete colour-blind,' after they know of their mistake and have practised themselves in distinguishing colours, will so comport themselves at a second trial that we have to simply mark them as of 'feeble colour-sense.' This fact might support Dr. Favre's idea that defective chromatic vision may be improved. This possibility, however, does not militate against our hypothesis from the theory, as to the nature of feeble colour-sense. It does not change our standpoint in the question. The same will sometimes happen with test No. II, and it is explainable by what we have said; namely, that, between the complete lack of chromatic sense and the incomplete, there is a series of gradations, and that in such cases practice would affect the result of examinations.

"All the examples given prove that many seeming trifles and stratagems are of value in making the examination—amongst others the keeping the sample a little way off from the heap of worsteds, as also the removal of everything which can cause the examined doubt and uncertainty. We must not, therefore, let them do what many want to do; namely, hold a number of the worsteds in the hand at once. We must make the person being examined place each skein, as he takes it up, either with the sample or else back on the heap. Many who are not clear whether the skein is like the sample or not, instinctively put the shades most resembling the test sample at the side of the

heap towards it, and thus gradually form a little bridge, but which for correctness they will not vouch for. No such half-measures must, however, be allowed.

"*Deciding whether the Examined are fitted for their Duty.*

"The method of scrutiny here described is able to detect, as we have seen, not only complete or incomplete colour-blindness, but a feeble chromatic sense. Moreover, it has been proved that there is a perfect gradation, from complete colour-blindness on the one side to the normal chromatic perception on the other. The question then naturally arises, from our practical point of view, whether it is possible to draw a dividing line between the kinds and degrees of defective colour-vision which would except those who could not cause any inconvenience to the railway service, and, in case of an affirmative answer, where such limit is to be found.

"It must first be remembered that, in the existing state of things, these questions neither can nor ought to be settled in the same manner in every case, since the examination is intended for individuals of two different classes—1st, the aspirants for railway employment; and, 2nd, the employés, or those already in service.

"It will be readily understood how great is the difference of the cases, in deciding what may be the result of the examination. We have already given our views on this point. Justice here calls for an essential distinction, supposing that the test has been always made with sufficient accuracy. Hence we must pay especial attention to both of the above classes when deciding whether an employé is fitted for his duty.

"(A) *Those who are Applicants for Railroad Service.*

"We must bear in mind that in Sweden, according to the regulation in force there for the management of state railways (followed also, as far as we know, on the private lines), it is required that, in order to be admitted, each applicant must 'prove by a certificate from a physician that he is exempt from any kind of infirmity, disease, or defect of conformation that could be prejudicial to the exercise of his functions;' and also, that among these defects of conformation, in connection with signals, are reckoned the defects of the chromatic sense, to which the managers have especially directed the attention of the medical men attached to the lines.

"According to the principles laid down, the greatest severity should be observed; or, in other words, the least defect in the sense of colours should be a sufficient ground for rejection.

"We must seek, therefore, to adapt the method of testing to this law. The object of a test is to prevent any one from working as a railroad employé who does not have a perfectly normal colour-perception. We have already sufficiently explained the evils arising from contrary action in case of admission to

railroad work. The border between normal and abnormal colour-sense, like that between the normal and abnormal in all analogous fields, is purely conventional, and can never be sharply defined. In this case, however, it is necessary, and our experience shows, that, so long as the question of improving colour-blindness is an open one, we must consider as over the border the slightest chromatic defect that our method can detect, or the slightest degree of incomplete colour-blindness; that is, feeble colour-perception. Considering the smallness of the defect the rule seems hard; and yet we think that it is not too severe. On the contrary, it is quite possible that hereafter still stricter rules may become necessary.

"Our practical work is greatly simplified by drawing this boundary-line. We hold as fixed that the surgeon is not to be asked to decide whether a man is fit for the service or not, but simply to state the kind and degree of the colour-blindness of the employé referred to him. The decision of an intelligent person is then immediate and decisive, whether he gives the examined a certificate, including the state of colour-vision, or refuses the latter. The statement of the slightest colour-blindness in the first case, as also the refusal to give a certificate in the latter, are both equal to refusal.

"(B) *Employés already in Service.*

'We must here ask ourselves if we must not modify the limit we have just traced, in order to carry out the principle we stated before; namely, that it is necessary to adopt less severe rules as to the elimination from the service of those who are already employed. We here encounter great difficulties; and it will be seen that it is not possible to settle the question summarily; that is, that a sharply defined limit cannot be traced. In such cases the physician should always, when he discovers a defect in the chromatic sense, give a certificate which will indicate its nature. These indications include, as we have already said, the diagnoses *complete red-blindness*, *complete green-blindness*, *incomplete colour-blindness*, *or a feeble chromatic sense.*

"Our method adheres strictly to the theory; but, on account of the transition-forms, the diagnosis cannot always meet the very exact demands of the theory. If we class with complete colour-blindness only those cases in which one of the three elements of the visual apparatus is wholly wanting or completely paralyzed, and with incomplete colour-blindness only those cases in which none of the three are wholly wanting, but simply the susceptibility of one is very much reduced, we shall have to group many cases of the latter class with the first. On the other hand, we shall often have to consider the lower grades of incomplete colour-blindness with feeble chromatic sense. We must, however, recall cases of a person—especially if he have subsequently practised himself—being at the first examination marked as completely colour-blind, whilst on a second time they

have appeared as only incompletely colour-blind; and others where a person was at one time incompletely colour-blind in the fullest sense of the word, whilst at another only feeble colour-sense could be shown. In such cases the record should state, in addition, 'incomplete colour-blindness,' approaching complete red or green, or incomplete colour-blindness of slight degree, &c.

"The same strict rule should be applied to those already employed as to those seeking service, and all should be discharged who show any lack of colour-perception. This would certainly most fully protect the railroad service from danger. Such a general law, however, has its difficulties, especially as we must recognise, in respect to the danger of confounding the signals, a great difference between complete colour-blindness and a feeble colour-perception. The different cases of incomplete colour-blindness vary also in degree. To draw a line here, and say beforehand who shall be dismissed and who retained, will be as easy in regard to the first as difficult in reference to the latter; for we are convinced that every case of complete colour-blindness of both kinds, as well as every case of incomplete of the higher degrees, should be immediately dismissed. But, as regards those who may be retained, it is clear that the first question concerns those who, at the time of the trial, were regarded in the diagnosis only as having a feeble chromatic sense, and then those who in the first test merely confound grey with the sample-colour. But we do not venture to lay this down as a principle; for, if it should be proved that these individuals can generally distinguish the light of coloured lanterns with sufficient accuracy, this does not prove that it is so in every case, and especially not at every distance such as are required in the service. This is why we know nothing better to advise than to refer all such cases to competent specialists, as long as the transition period of which we have spoken lasts.

"It may be asked, How will the specialists themselves proceed? To answer this, however, would require a much more extended scientific discussion of the various methods than we have proposed here to make. We would only give some hints. A specialist who is familiar with this subject has all known methods at his disposition; and, if these fail, he need but invent others. As, however, I have been in the position of the specialist in reference to the reform on the railroads of Sweden, I will here say how I have proceeded.

"In the examination of doubtful cases submitted to my judgment, I determined according to several of the methods mentioned in one of the preceding chapters. In general, these persons were all subjected to a trial according to the methods of Seebeck and Maxwell, and an examination by means of the visual perimeter and of coloured shadows, as well as the lanterns of my invention and coloured glasses. These last means have capacity especially in view; and they are very suitable for the object, when it is desired to investigate those who have

been already discovered, by my method of Berlin worsteds, as having a defective chromatic sense.

"The light of coloured lanterns and illuminated surfaces generally, conveniently arranged and methodically used, may serve especially in such cases to enlighten us as to the faculty of the person examined for appreciating coloured signals. Our experiences of this kind have shown us that the majority of colour-blind railway employés, however much practice they have had, are utterly incapable of recognising and distinguishing hed regulation colours of lanterns, especially when they are employte in the shades which are not most commonly in use in the service. This applies not only to the completely red- and green-blind, but also to the incompletely blind. These last require the most circumstantial investigation, and it is not to be assumed that the lower degrees can stand the trial. They may often, it is true, distinguish the signal-lights at a short distance with sufficient accuracy; but they do not succeed at a comparatively greater distance. As the places where the trials are usually made do not command such distances as railways for observing signals, signal-lights cannot of course be used for these trials. They are replaced by small illuminated surfaces, which, seen from a suitable distance, produce exactly the same effect as lanterns at a great distance. Such surfaces are made by placing a screen, with a suitable opening covered with a coloured glass, before the flame of a lamp.

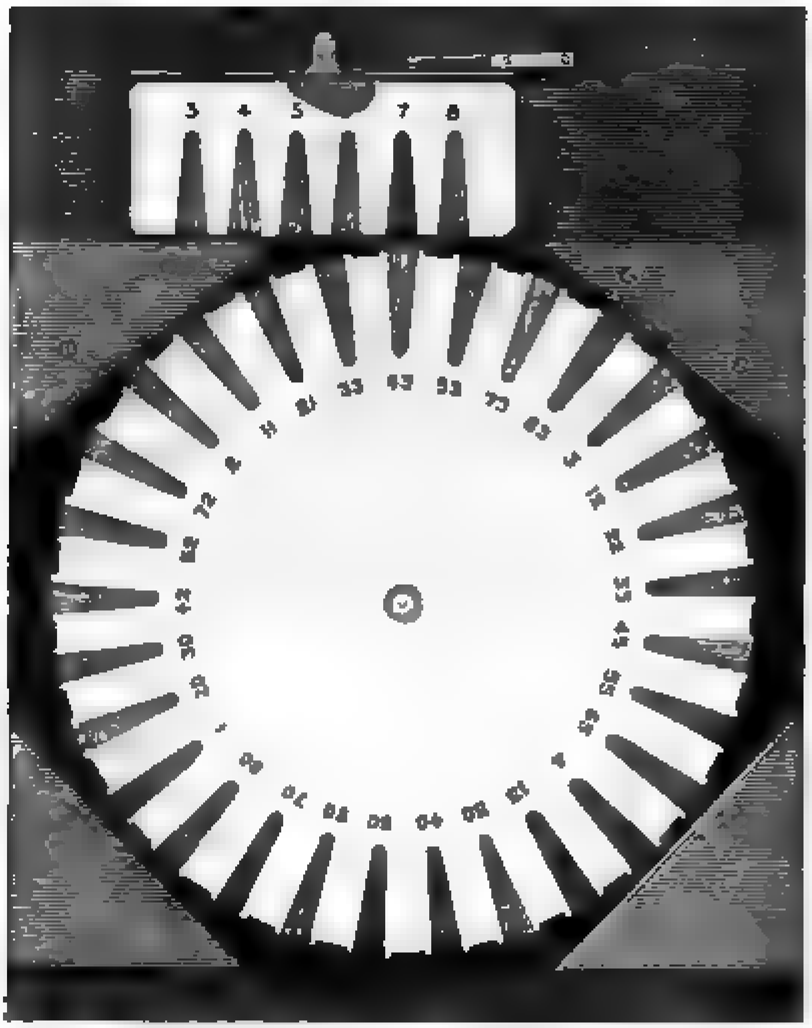
"We have, however, said enough in reference to means to be employed in such cases. We had no wish to enter into further details, and doubt whether this would on the whole be advisable."

Appendix IV.

Dr. Jeaffreson's Test Disc.

Dr. Jeaffreson's test apparatus consists of a rotating celluloid disc, about a foot in diameter, upon which skeins of wools are arranged radially at the outer edge (*see* Fig. I). All of the

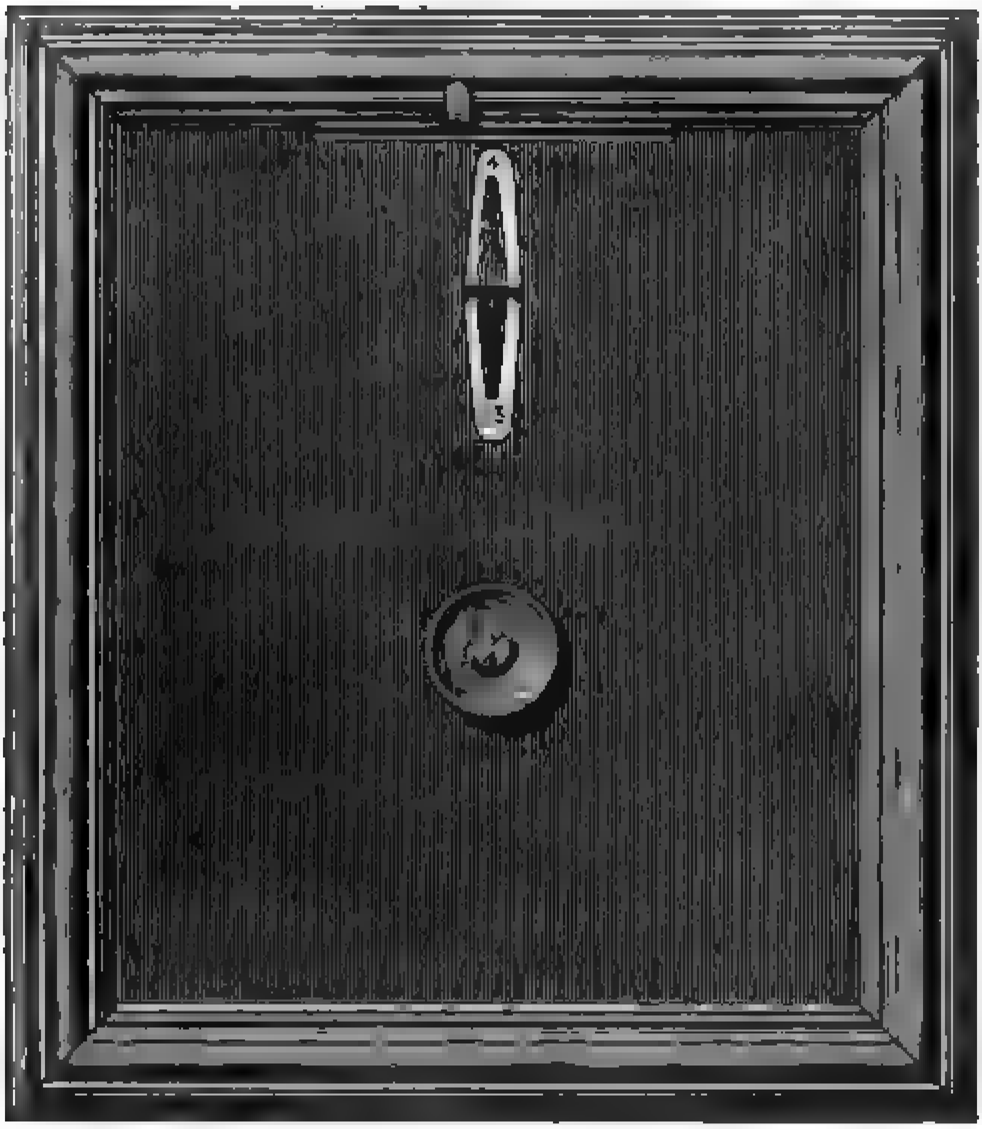
Fig. I.



disc except a small aperture, as shown in Fig II, is covered. By means of a button attached to its centre, the disc can be turned until any colour is brought opposite to that standard test colour which is seen in the upper aperture. The test skeins are the three Holmgren test colours, and a yellow, blue, and purple. The apparatus is mounted in a frame, so that it can be hung upon the wall.

In using the test the usual course is to point out to the person under examination the pale green wool in the upper aperture, and

FIG. II.



request him to turn the button until he brings several skeins of what appear to him to be the same colour on the disc opposite to the one he has to match. When the examination with this colour is completed, the pink skein is proceeded with in the same manner, and this is followed by the other test colours, if considered necessary, following it, if desired, with from one to twenty confusion colours. The colours on the disc which are chosen can be registered by numbers for future reference, or for comparison with the results of a second examination, where, in case of disputes, it is called for.

APPENDIX V.

TEST WITH THE SPECTROSCOPE.

The test with the spectroscope requires an apparatus somewhat complicated in construction, and therefore expensive, but it should be applied when an appeal from the verdict of the examiner is made.

In any examination it is essential that both the examiner and the examinee should see the test colours at the same time, or at least that the former should by some means know exactly what is being shown. For a simultaneous view it is advisable that the spectrum should be formed by a source of light at least as bright as the lime-light, when it has to be thrown upon some white reflecting surface. If the apparatus be so made that a patch of monochromatic light from any part of the spectrum can be thrown upon some one spot of the white receiving surface the examination will become easy, more particularly if a patch of white light can also be thrown separately or together with the monochromatic light on the same spot, as this enables any dilution of the pure spectrum colour to be effected, and gives a means of detecting imposition. As already pointed out, every decidedly colour-blind person sees some one part of the spectrum as—what he calls—white. If the spectrum colours alone were thrown on the screen, it is quite possible that the examinee might be taught that when a colour which formed the patch appeared white to him he ought to call it green or bluish-green, and thus detection of the imposture would be difficult.

But if the mode of testing be arranged as follows, the difficulty would be overcome:—

A patch of any coloured light should be thrown on the screen, and the candidate asked to indicate if it was white. The colour might then be diluted with white, and the question again asked. A pure white patch might then be put on the screen and the question repeated. The colours should be gradually changed until his neutral point was approached. At this place the colour seen as white would be mistaken for white, as the changes would be made by dilution or by omitting the colour altogether. This test involves no naming of a colour, but only a knowledge of white. The discovery of a neutral point would infallibly indicate that the candidate was colour-blind.

Another simple test is to mix three spectrum colours to form white, one of the rays being situated near the neutral point of the red-green blind. The white would be the same to the colour-blind as to the normal eye, and it would still remain white to the colour-blind whether the colour at the neutral point were increased or diminished. No amount of coaching would enable the examinee to make constantly correct answers.

By placing a bull's-eye of a lantern in this patch, and by arranging that the three colours, the blue-green of the neutral point, a green closer to the red, and a red, and also the white should all have about the same luminosity, a further test in imitation of signal lights could be carried out.

The question as to the character of the colour-blindness need not be investigated; but if a patch of light from the extreme red of the spectrum were thrown on the screen, and diluted slightly with white light, the green-blind would see it coloured red or yellow, whilst the red-blind would see only the white.

An instrument based on the principle of Clerk-Maxwell's colour-box could also be used in much the same way as indicated above, but in this case the examiner would not see the patch of light, and could only examine the case after the positions of the different colours had been accurately determined beforehand.

Appendix VI.

Form Test.

All tests of form-vision depend upon the principle that the magnitude of the image formed upon the retina, by any object, depends partly upon the magnitude of the object itself, and partly upon its distance from the observer; or, in other words, upon the magnitude of the visual angle which it subtends, while the retinal image must itself attain a certain magnitude before the object from which it is derived can be clearly seen. The precise character of the test object is not important, and perhaps the best is furnished by groups of equal circular dots, each one separated from its neighbours by an interval equal to its own diameter. For all practical purposes, however, printed letters are sufficient, and it is found by experience that capital letters, in block type, are easily distinguished by the majority of mankind when they are placed at such a distance that each limb or part of a letter is seen under a visual angle of one minute, and each letter as a whole under a visual angle of five minutes. Sets of "test-types" were first made on this principle by Dr. Snellen, of Utrecht, and are commonly called by his name. They consist of lines of letters of different sizes, each size marked by a number, which corresponds with the number of feet or metres of distance at which it will subtend the visual angles mentioned above, and at which it should therefore be clearly legible. The acuteness of vision is expressed by a fraction, of which the numerator is the distance of the observer from the tests, while the denominator is the number of the smallest letters which he can read at that distance. Thus if at 20 metres he can read No. 20, he is said to have $\frac{20}{20}$, or normal vision; but if at 20 feet he can only read No. 40, or if, in order to read No. 20, he finds it necessary to approach within 10 feet, he would, in the former case, be said to have $\frac{20}{40}$, and in the latter $\frac{10}{20}$, of normal vision, in either his vision being equal to $\frac{1}{2}$. The test is rapidly applied in practice by hanging up a sheet of properly constructed letters in good daylight, by placing the person to be tested at a measured distance from them, and by desiring him to read the smallest he can. The letters may be procured from any optician, and, in testing large numbers of people, it is desirable to have some mechanical contrivance for concealing part of each line, so that the examiner may not be deceived by the lines having been previously learnt by heart by the examinees.

Appendix VII.

Summary of Colour-Blind Cases detected at the examination of about 300 Railway Employés at Swindon on 22nd June, 1891.

Explanation.—The names at the head of the columns are those of the Examiners. G = Green, R = Red, indicating the colour-perception which was deficient or entirely absent. The mark — shows that the Examinee was passed by the Examiners.

Examinee's Number.	Wool Test.		Lantern Test.				
	(a.) Mr. Mellish.	(b.) Capt. Abney.	(c.) Capt. Thompson.	(d.) Dr. Edridge Green.	(e.) Mr. Galton.	(f.) Mr. Nettleship.	(g.) Mr. B. Carter.
4	? G	? G				? G	
12						? R	
41	R	? G	—		? G R	? G R	
43						? R	
49						? G	
50	G R					—	G
60	G R		G			G	G
69	R		—			? G R	
82						? G	
88		—				? G	
98						? G	
122	G	G R		*	—	—	
129						? G	
133						? G	
191				—			R G
202	—					? G	
209		—				? R	
218						? G	
311			—		—	? G	
327						? G	
543						R	
556		—				? G	
569						? G	
573						? G	
621						? R	
634			G			? G R	
641	—			†			
642						? G	
652	G R	G	—		? G	G R	G
718	R				—		G R
724	R G		R G		R G		R G
904	R		G				R G
Payne	R G	R G	R G				
Hext	R	R G				R G	R G
Total Number Examined	138	88	101	8	66	78	8

(*a*) This column includes those examined by Mr. Brudenell Carter and Dr. Frost, who also used the Holmgren tests.

(*b*) The cases given in this column were detected by Dr. Jeaffreson's wool test apparatus.

(*c*) Capt. Thompson used the Board of Trade tests—lamp and glasses.

(*d*) Dr. Edridge Green's lamp did not arrive at Swindon early enough to enable him to examine more than eight men.

(*e*) Mr. Galton used a very convenient lamp of his own design. After the proceedings at Swindon, Mr. Galton wrote. "The wool test is surer than the lantern test and more convenient."

(*f*) Mr. Nettleship in transmitting the results of his examination states that the lantern test as he used it "is evidently quite untrustworthy as a first test, though it may perhaps have value as a test of practical efficiency when the real colour state has already been determined."

(*g*) This column gives a few cases tested by Mr. Carter with his own pattern lantern after he had finished with the wool test.

* *Called Yellow*, red. Rejected.

† *Called Green*, blue; and *White*, red. Rejected.

April 28, 1892.

Mr. JOHN EVANS, D.C.L., LL.D., Treasurer and Vice-President, followed by The LORD KELVIN, President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On a Decisive Test-case disproving the Maxwell-Boltzmann Doctrine regarding Distribution of Kinetic Energy." By The LORD KELVIN, Pres. R.S. Received April 6, 1892.

The doctrine referred to is that stated by Maxwell in his paper "On the Average Distribution of Energy in a System of Material Points" ('Camb. Phil. Soc. Trans.,' May 6, 1878, republished in vol. 2 of Maxwell's 'Scientific Papers') in the following words:—

"In the ultimate state of the system, the average kinetic energy of two given portions of the system must be in the ratio of the number of degrees of freedom of those portions."

Let the system consist of three bodies, A, B, C, all movable only in one straight line, KHL:

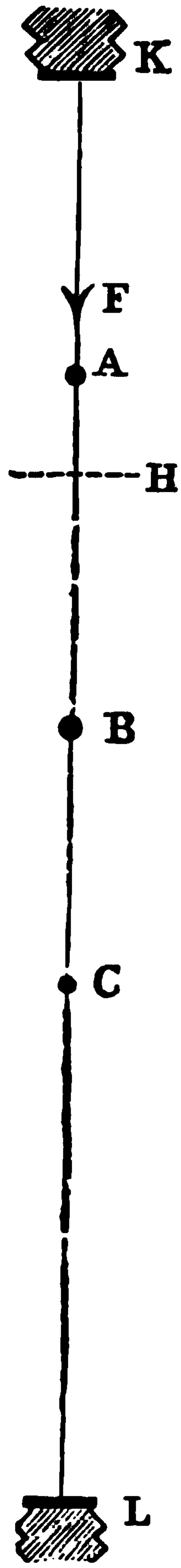
B being a simple vibrator controlled by a spring so stiff that when, at any time, it has very nearly the whole energy of the system, its extreme excursions on each side of its position of equilibrium are small:

C and A, equal masses:

C, unacted on by force except when it strikes L, a fixed barrier, and when it strikes or is struck by B:

A, unacted on by force except when it strikes or is struck by B, and when it is at less than a certain distance, HK, from a fixed repellent barrier, K, repelling with a force, F, varying according to any law, or constant, when A is between K and H, but becoming infinitely great when (if at any time) A reaches K, and goes infinitesimally beyond it.

Suppose now A, B, C to be all moving to and fro. The collisions between B and the equal bodies A and C on its two sides must equalise, and keep equal, the average kinetic energy of A, immediately before and after these collisions, to the average kinetic energy of C. Hence, when the times of A being in the space between H and K are



included in the average, the average of the *sum of the potential and kinetic energies of* A is equal to the average kinetic energy of C. But the potential energy of A at every point in the space HK is positive, because, according to our supposition, the velocity of A is diminished during every time of its motion from H towards K, and increased to the same value again during motion from K to H. Hence, the average kinetic energy of A is less than the average kinetic energy of C!

This is a test-case of a perfectly representative kind for the theory of temperature, and it effectually disposes of the assumption that the temperature of a solid or liquid is equal to its average kinetic energy per atom, which Maxwell pointed out as a consequence of the supposed theorem, and which, believed to be thus established, has been

largely taught, and fallaciously used, as a fundamental proposition in thermodynamics.

It is in truth only for an approximately "perfect" gas, that is to say, an assemblage of molecules in which each molecule moves for comparatively long times in lines very approximately straight, and experiences changes of velocity and direction in comparatively very short times of collision, and it is only for the kinetic energy of the translatory motions of the molecules of the "perfect gas," that the temperature is equal to the average kinetic energy per molecule, as first assumed by Waterston, and afterwards by Joule, and first proved by Maxwell.

II. "Researches on Turacin, an Animal Pigment containing Copper: Part II." By A. H. CHURCH, M.A., F.R.S., Professor of Chemistry in the Royal Academy of Arts, London. Received April 2, 1892.

(Abstract.)

This paper is in continuation of one read before the Society in May, 1869.*. It contains an account of observations made by other investigators on turacin and on the occurrence of copper in animals; a table of the geographical distribution of the Touracas, and a list of the twenty-five known species; a chart of turacin spectra (for which the author is indebted to the kindness of Dr. MacMunn); and a further examination of the chemical characters and the composition of turacin. The more important positions established by the present inquiry are these:—

1. The constant occurrence in eighteen out of the twenty-five known species of *Musophagidæ* of a definite organic pigment containing, as an essential constituent, about 7 per cent. of copper.

2. The "turacin-bearers" comprise all the known species of the three genera, *Turacus*, *Gallirex*, and *Musophaga*, while from all the species of the three remaining genera of the family *Musophagidæ*, namely, *Corythæola*, *Schizorhis*, and *Gymnoschizorhis*, turacin is absent. Furthermore, the zoological arrangement of the genera constituting this family is in accord with that founded on the presence of turacin.

3. The spectrum of turacin in alkaline solution shows, besides the two dark absorption bands previously figured, a faint broad band on either side of line F, and extending from λ 496 to λ 475.

4. The spectrum of *isolated* turacin in ammoniacal solution shows, besides the three bands already named, a narrow fourth band, lying on the less-refrangible side of line D, and extending from λ 605 to

* 'Phil. Trans.,' vol. 159, pp. 627—636.

λ 589. It probably arises from the presence of traces of the green alteration-product of turacin formed during the preparation of that pigment in the isolated condition, an alteration-product which is likely to prove identical with Krukenberg's turacoverdin.

5. Turacin in ammoniacal solution remains unchanged after the lapse of twenty-three years.

6. Turacin in the dry state, when suddenly and strongly heated, yields a volatile copper-containing red derivative, which, though undissolved by weak ammonia-water, is not only soluble in, but may be crystallised from, ether.

7. Turacin in the dry state, when heated in a tube surrounded by the vapour of boiling mercury, becomes black, gives off no visible vapour, is rendered insoluble in alkaline liquids, and is so profoundly changed that it evolves no visible vapour when afterwards strongly heated.

8. The accurate analysis of turacin offers great difficulty. The percentage composition, as deduced from those determinations which seem most trustworthy, is—

Carbon	53·69
Hydrogen	4·60
Copper	7·01
Nitrogen	6·96
Oxygen	27·74

These numbers correspond closely with those demanded by the empirical formula $C_{82}H_{81}Cu_2N_9O_{32}$, although the author lays no stress upon this expression.

9. Turacin presents some analogies with hæmatin, and yields, by solution in oil of vitriol, a coloured derivative, turacoporphyrin. The spectra of this derivative, both in acid and alkaline solution, present striking resemblances to those of hæmatoporphyrin, the corresponding derivative of hæmatin. But copper is present in the derivative of turacin, while iron is absent from its supposed analogue, the derivative of hæmatin.

III. "On the Mathematical Theory of Electro-magnetism." By Alex. McAulay, M.A., Ormond College, Melbourne. Communicated by the Rev. N. M. Ferrers, D.D., F.R.S. Received January 8, 1892.

(Abstract.)

It will conduce to clearness to give some account here of the objects and aims of what is to follow. The part of the paper succeeding this Introduction is in three main divisions: *The groundwork*

of the theory, The establishment of general results, and *The detailed examination of these results.*

The groundwork of the theory, though not the longest of these, calls for most attention here. It is divided into two parts—*Fundamental assumptions* and *Preliminary dynamical and thermodynamical considerations.* I do not propose to give here a *résumé* of the different parts, but to call attention to certain prominent features.

The two most important of the fundamental assumptions are, perhaps, first, that in all cases $4\pi C = V\nabla H$, which I take to be one of the most characteristic features, if not the most characteristic, of Maxwell's theory; and, secondly, that the modified Lagrangian function per unit volume, though of course it contains H, does not contain any term involving magnetic moment per unit volume or magnetic induction. Neither of these assumptions seems to be at variance with Maxwell's, and, as hinted, the first is taken up mainly because it is a fundamental feature in his theory. From the first it follows that C must obey the laws of incompressibility, and this naturally leads to the assumption that D also invariably obeys those laws. The second leads to very important consequences, which I believe have not before been traced, and which I wish to call attention to here. Though not put quite in this form below, they amount to this, that ${}_H\nabla l$, where l is the modified Lagrangian function per unit volume of the standard position of matter, obeys the law of incompressibility, that round every circuit there is an electromotive force equal to the rate of decrease of the surface integral of $4\pi{}_H\nabla l$ through the circuit, and that ${}_H\nabla l - H/4\pi$ appears in subsequent equations in such a manner as to compel us to identify it with the magnetic moment per unit volume.* It is clear then that $4\pi{}_H\nabla l$ is, according to the present theory, the magnetic induction. As the theory is developed below, it is convenient to define B as equal to $4\pi{}_H\nabla l$, and call B the magnetic induction, leaving the justification till we examine the detailed consequences of the theory. It is well to insist on this result here, as it does not appear obvious in the work below, but only comes out when a general review of a great part of the paper is made. To put the matter in the form of a proposition:

If the two fundamental assumptions are made (1) *that* $4\pi C = V\nabla H$ *and* (2) *that l, the Lagrangian function per unit volume, can be expressed in terms involving* H, *but independent of magnetic induction and of magnetic moment per unit volume, then the magnetic induction must be* $= 4\pi{}_H\nabla l$.

* Strictly speaking, the last clause should be modified by the condition "if the present position be taken as the standard position." This, however, is only an accident of the particular form of enunciation, which at the present stage is unavoidable.

The other most important features of the fundamental assumptions are first those already described with reference to the electric co-ordinates, and the expression for the current in terms of the displacement, and, secondly, the manner in which are treated the two currents, conduction and dielectric (the latter being inappropriately on the present theory denominated the "displacement current"). If there are (and physicists seem agreed on the point) two independent currents whose *sum* appears in the equation $4\pi C = V\nabla H$, and whose *sum* obeys the laws of incompressibility, it seems to me of the nature of a truism that there must be also two independent electric displacements, whose *sum* obeys the laws of incompressibility. I, therefore, from the very beginning recognise two displacements, d and k, which I call, for want of better names, the dielectric and conduction displacements.* This naturally leads to the contemplation of two independent kinds of electro-motive force. This last, however, is subsequently satisfactorily disposed of.

Before leaving the fundamental assumptions let me remark that, though in some important respects the present theory may seem to differ from Maxwell's, it will be found, I think, that just where the difference seems to be most marked, is Maxwell's theory most vague. All the differences, if they really be such, have been forced on me unwillingly in the attempt to put into definite form what I take to be the essence of Maxwell's theory. At any rate the results, though not in every respect identical with Maxwell's, are yet so nearly identical that the true matter for surprise is that they differ so little and in such unimportant ways from his.

It must be added, to prevent misconception of my own views, that I by no means consider proven, what I regard as the key to Maxwell's theory, and what I have strictly adhered to in this paper, the assumption that under all circumstances $4\pi C = V\nabla H$. My position rather is that, while the assumption may or may not be true, it is desirable to investigate as generally as possible what must be true, and what cannot be true if the assumption is made. In other words, I do not think that Maxwell's theory has yet had a fair trial, even at the hands of mathematicians, and the present paper is an attempt to provide more ways and means than hitherto have been available, for such a trial. The methods adopted are equally applicable to other sets of fundamental assumptions.

* Perhaps it would be better to call them the *elastic* and *frictional* displacements, or the *reversible* and *irreversible* displacements. I wish to leave this point open for those better qualified to decide. Of the three sets of terms suggested above, the last seems to me the best. The only reason for adopting in the present paper the names given in the text is to imply the origin of the assumption that there are *two* such displacements. Of course, if we call the two displacements reversible and irreversible, we must also call the corresponding currents reversible and irreversible.

Turning to the second part of the groundwork, the *preliminary dynamical and thermodynamical considerations*, it is necessary to remark that these considerations, though not limited to an electric field, seemed absolutely necessary in order thoroughly to investigate the consequences of the assumptions. With regard to the first two sections of this part of the paper on *the modified kinetic energy and the free energy*, and on *the entropy*, there is nothing which is likely to be questioned. In the third section on *frictional forces, conduction of heat and dissipation of energy*, I enunciate a principle which opens the way for much criticism. I would beg any readers, to whom the form of enunciation is repugnant, to suspend their judgment as to the validity of the principle not only until the first justification of it, but until they have seen it in action, as it were, later in the paper. What was wanted was to bring this group of phenomena, which are undoubtedly closely connected, under the same sort of treatment as is accorded to the reversible phenomena of a system by means of its Lagrangian function and the (dependent) entropy.

The way being thus paved, in the next principal division of the paper are deduced the general results of the theory, the most important of which are the equations of motion. These are considerably more general than the ordinary equations of the field, and thus we are led to the last division of the paper, the detailed examination of these results. The chief sub-divisions of this part are the comparison with Maxwell's results, a discussion from the point of view of the present theory of thermo-electric, thermo-magnetic, and the Hall effects, and the transference of intrinsic energy through the field.

In comparing with Maxwell's results, wherever there is agreement it is considered unnecessary to investigate further the detailed consequences. Where there is disagreement the physical consequences are traced with more detail, and in no case can it, I think, be said that the results of this part of the paper are condemnatory of the present theory. In this place, too, the bearing of the present theory on the question of convection currents is discussed.

Perhaps a clearer insight into the true bearings of the present theory is obtained by the attempt below to explain thermo-electric, thermo-magnetic, and the Hall effects, than by any other part of the paper. Especially clearly do some of the restrictions imposed by the condition $4\pi C = V\nabla H$ come out.

In the last sub-division it will be found that I disagree entirely with Professor Poynting's interpretation of his own results, and show how quite a different, and I think simpler, flux of energy may be made to account for the changes of intrinsic energy in different parts of the field. In particular, this interpretation would restore credence in what Professor Poynting considers he has shown to be a false view, viz., that among the aspects of a current of electricity it may be

looked upon as something conveying energy along the conductor. This part of the subject, although deduced from the present theory, is shown to be true on Professor Poynting's own premisses.

It is well here to call attention to what might prove confusing otherwise. In what follows E, e, F, Φ, and some allied symbols stand for certain external forces. But there are three different meanings given in different parts of the paper to these symbols. They are originally defined as the whole external forces of the different types. But in treating of frictional forces, &c. (§§ 35 to 42), it is convenient to regard them as meaning only those parts of the forces which are *due to* friction and the like. Again, from § 50 onwards, it is convenient to regard them as meaning only those parts of the forces which are *independent of* friction and the like. This inconvenience is incurred to avoid the greater evil of a large additional array of symbols.

With this exception,* and one or two other trifling ones, which are noticed in their places, nowhere has the meaning of a symbol been changed throughout the paper.

IV. "Stellar Photometry." By W. J. Dibdin, F.I.C., F.C.S., &c. Communicated by A. Vernon Harcourt, F.R.S. Received February 23, 1892.

(Abstract.)

Hitherto the determinations of stellar luminosity have been made solely with regard to the relative intensity of the stars apart from any reference to a known terrestrial unit. The various methods which have been employed do not denote actual intensity, and the present inquiry was, therefore, undertaken with the view of elucidating this question, especially with regard to those stars whose colour has presented a difficulty.

As a preliminary, the author prepared a standard series of artificial stars of various colours and known intensity, in terms of the English standard candle. These range in value from one candle to 0·000018 candle, and when placed at a distance from the telescope form a standard series for comparison.

The evaluation of the coloured lights was made by means of the author's modified "star" disc, by the use of which comparisons of various coloured lights can be readily obtained.

* Since completing the paper I have discovered a notable exception, which is *not* otherwise noticed than in this foot-note. It does not seem likely to lead to confusion; therefore I retain it. Most frequently in the present paper q stands for the typical *scalar* coordinate of a dynamical system, but it is not infrequently used, as in the former paper, for the *quaternion* of the rotation-operator $q(\ )q^{-1}$.

The second portion of the inquiry was directed to the most suitable means of comparing a star with the standard. Such a method must provide for the estimation of the total luminous energy, irrespective of the fact that in the case of a star the light practically emanates from a point, whilst that from the standard emanates from a surface. This fact precludes the use of the wedge photometer.

The methods employed by the author were three in number. The first was by means of reflection from a plane mirror mounted in front of the telescope object glass, in such a manner that the light of one of the radiants is seen by reflection through one half of the lens, whilst that of the other is viewed through the other half, by direct vision, both radiants being thus simultaneously seen in the same field of view. When the two images are placed out of focus to an equal extent, so as to be equal in size, comparisons of intensity can be made. By this system it matters not whether the light emanates from a point or a surface, as it is the degree of illumination of the respective portions of the object glass which is measured. Errors due to the unequal transmitting power of the different portions of the lens, and also those due to the loss of light by reflection, &c., are corrected by repeating the observations after the whole of the apparatus has been reversed; *i.e.*, if the star is seen by reflection in the first observation, it is to be viewed by direct vision in the second, when the standard light will be viewed by reflection.

The second method employed was a modification of Zöllner's lamp, the standard used being the author's pentane Argand burner, in which air carburetted with pentane is employed as a combustible. This is mounted on the eye-end of the telescope. Next to the burner is placed a plate of ground blue glass, carefully adjusted to reduce the colour of the gas flame, so that it is, as nearly as possible, comparable with that of daylight. This glass is mounted permanently in front of an aperture, 0·08 inch in diameter, in a brass plate. At a distance of 2·4 inches from this is placed a second plate of brass, having an aperture of 0·013 inch diameter. A lens, fixed at 2 inches from this second plate, projects the image of the illuminated surface of the blue glass by a reflecting prism, to the eye-piece of the telescope, in which it is viewed as a circular disc of light, side by side with that caused by the illuminated image of the telescope object glass. The ground surface of the blue glass affords a slightly granulated appearance, exactly imitating that of the object glass when a star is viewed out of focus. By arranging the position of the eye-piece until the two images are exactly of the same size, the imitation of a star is so striking that it is all but impossible, when the colours are alike, to discern the difference.

For the modification of the colour of the comparison light, two series of coloured glasses are arranged in rotating diaphragms situated

between the two perforated plates 2·4 inches apart. These can be conveniently brought into position for modifying the colour and intensity of the comparison light. The coefficient of absorption of each of the coloured glasses was determined photometrically, and tables prepared by reference to which corrections for their use can be made.

As this method was found to present an objectionable feature in regard to the uncertainty attending the use of multiple glasses for reducing intensity, the third method was employed. The lamp was placed on a graduated bar in such a manner that its distance from the ground blue glass could be conveniently altered, and the illumination of that glass reduced on strictly photometrical principles. This method was found to answer so well that the experimental apparatus first employed is being altered and improved. When it is completed, a further and extended series of observations will be made. In the meantime, the results already obtained may be discussed.

Details of the observations are given in the tables presented with the paper. By plotting the average results on a diagram a mean curve is drawn. From the value thus found, the relative intensity of stars of all other magnitudes can be calculated.

It then only remains to convert the comparative into actual values by the correction for the distance of those stars whose positions are known, when their actual intensities will be ascertained.

The following series is given of the average results of the determinations of the intensity of a sequence of stars in descending order of brightness, together with their respective magnitudes, and a comparison of their theoretical intensities on the assumption that a second magnitude star equals 0·00075 candle placed at a distance of 109 feet, which factor is deduced from the mean curve of all the determinations made :—

Average Results by Methods 1, 2, and 3.

Star's designation.	Magnitude. Pritchard.	Illuminating power found. Candle at 109 feet.	Theoretical illuminating power on the assumption that mag. 2 = 0·00075 candle at 109 feet.
Vega	+0·86	0·0039	0·0041
Capella	0·08	0·0017	0·0020
Aldebaran	1·12	0·0015	0·0017
γ Orionis	1·79	0·00075	0·00090
β Tauri	1·79	0·00085	0·00090
β Aurigæ	1·94	0·00125	0·00080
δ Orionis	2·02	0·00074	0·00074
Polaris	2·05	0·00081	0·00072
α Andromedæ	2·05	0·00085	0·00072
β Ursæ Minoris	2·26	0·00045	0·00059
γ Cygnus	2·26	0·00035	0·00059
α Pegasi	2·33	0·00062	0·00055
γ "	2·47	0·00031	0·00048
γ Andromedæ	2·72	0·00042	0·00038
θ Aurigæ................	3·03	0·00018	0·00029
γ Ursæ Minoris	3·02	0·00029	0·00029
ε " "	4·46	0·000040	0·000080
δ " "	4·54	0·000044	0·000070
ζ " "	4·65	0·000025	0·000065
B.A.C. Cat. 6754 Cygni....	5·27	0·000015	0·000037
24 Ursæ Minoris	5·87	0·000013	0·000021

The atmospheric absorption of light is discussed, and instances given of the considerable diminution in the light of a star when no apparent cause was discernible to ordinary observation.

Eight determinations of the light of Jupiter have been made, the average result being to ascribe to that planet a light equal to 0·020 candle placed at a distance of 109 feet. Determinations of the light of the planet Venus and of the Moon are also given, and the necessity for further observations referred to.

The total quantity of light afforded by the stars (apart from the planets) is calculated from the above results combined with Argelander's estimate of the number of stars down to the ninth magnitude. Assuming that the light afforded by these is equalled by the innumerable stars of lesser magnitude than the ninth, and by nebulæ, total starlight will equal that from 1·446 candles when placed at a distance of 109 feet. If it be further assumed that only one-sixth of the stars are capable of illuminating a given surface at the same moment, then such illumination will be equal to that afforded by one standard candle placed at a distance of 210 feet.

V. "On Some Phenomena connected with Cloudy Condensation." By JOHN AITKEN, F.R.S. Received March 2, 1892.

In the first part of this communication I intend giving the results of an investigation into the phenomena connected with the cloudy condensation produced when a jet of steam mixes with ordinary air, with special reference to the marked change which takes place in the appearance of the jet by electrification and other causes. In the second part will be given the results of an investigation into certain colour phenomena which can be produced when the condensation is made to take place under certain conditions, and it is thought that these experimental colour phenomena, if they do not give the explanation of a "green sun," at least enable us to reproduce it artificially with the materials existing in our atmosphere.

PART I.—STEAM JETS.

When a jet of steam escapes into the air, condensation at once ensues by the expansion and the mixing of the steam with the cold air. The result is the jet becomes distinctly visible by the light reflected by the minute drops of water carried along in the mixed gases and vapour. At first sight there is little that is interesting in the changes then taking place. The subject has, therefore, attracted but little attention, and has been but little studied. This is evident from the great interest that has been taken in the change produced in the appearance of the jet when it is electrified; yet I hope to be able to show that this is only one of a number of causes which alter the appearance of the condensing steam.

R. Helmholtz was the first to show that when an ordinary jet of steam is electrified, there is a marked increase in the density of the condensation. The effect of the electricity is certainly very remarkable. The instant the jet is electrified, it at once changes and becomes much denser, and the condensed particles also become visible much closer up to the nozzle from which the steam is escaping. For the convenience of description we shall call this second form of condensation *dense condensation*, while that usually observed we shall call *ordinary condensation*. Not that there is any hard and fast line between these two forms, as the one may be made to change by imperceptible degrees into the other. All that is meant is that the one is dense compared with the other.

One result of this investigation is that, in addition to electrification of the jet, there are four other ways in which the ordinary condensa-

tion may be changed into the dense form. These five ways of changing the ordinary into the dense form of condensation are:—

1. Electrification of the jet.
2. An increase in the number of dust nuclei.
3. Cold or low temperature of the air.
4. High pressure of the steam.
5. Obstructions in front of the jet and rough or irregular nozzles.

We shall now describe some experiments to illustrate each of these different ways of causing the ordinary condensation to change and take the dense form. In the experiments to be described, the steam was generally generated in a copper boiler, which could be pressed up to fully one atmosphere. The nozzle from which the steam escaped was placed at some distance from the boiler to prevent the hot gases influencing the jet. The steam was conveyed by means of a metal pipe to the nozzle, and a water trap was placed near the end of this pipe to prevent the irregularities which would be produced if the water condensed in the pipe were allowed to issue from the nozzle. The nozzle generally used was made of brass, carefully bored to a diameter of 1 mm., the diameter of the bore widening inwards, while the outside of the nozzle was turned to a fine edge in front. With this apparatus most of the experiments were made, but occasionally glass vessels and nozzles were used, as well as vessels and nozzles of other materials, but with no marked difference in the results.

1. *Electrification.*

In the experiments with electricity only steam of a low pressure should be used. The reason of this will be understood from what follows under division 4. In these experiments slight electrification was used, as only an old-fashioned cylinder electrical machine was available for the purpose, and in the damp atmosphere produced by the steam jet the electrification was only capable of giving a spark of about 1 cm. or generally less.

The necessary condition for the electricity producing any effect on the jet is that the particles in the jet be electrified either by direct discharge or by an induction discharge. The mere presence of an electrified body near the jet has no influence whatever. In order that it may have an effect, the electrified body must terminate in a point placed near the jet, and the potential must be great enough to cause a discharge of the electricity to the jet. When this takes place the jet at once becomes dense and remains in that condition while the discharge continues. The electrified body may, however, electrify the jet by induction. If, for instance, the electrified body be a sphere, and the nozzle from which the steam is issuing be pointed, the electricity discharged by the nozzle will electrify the

particles, and the condensation becomes dense. But if the nozzle be not pointed, then the presence of the electrified body produces no change, as there is no discharge of electricity. But if now we hold a needle or other pointed conductor near the jet issuing from the rounded nozzle it at once becomes dense, by the induction discharge from the point. In place of a point in the last experiment, we may use a flame; in fact, we may use any influence which will enable the electrified body to electrify the particles in the jet.

Another way of making this experiment is to insulate the boiler, and electrify it. If the nozzle be pointed, the jet becomes dense on electrification; but, if it be rounded, the electrification has no effect. If, however, we bring a needle or a flame near the rounded nozzle, the jet becomes dense. To get no effect from the electrification it is necessary that the nozzle be a ball of some size, the orifice through which the steam issues being, of course, the same diameter as that of the pointed jet.

The effect of the electrification has been studied by R. Helmholtz and by Mr. Shelford Bidwell,* but neither of them seems to be satisfied with any explanation they offer. Mr. Bidwell, from a spectroscopic examination of the light transmitted through the jet under the two conditions, came to the conclusion that in the dense condition the particles were larger than in the ordinary form of condensation; and he thinks that the increase in size is due to the electricity causing the small drops of water to coalesce and form larger drops. In support of this explanation, he quotes Lord Rayleigh's experiments on the coalescence of drops in water jets while under the influence of electricity. As Mr. Bidwell does not put forth this opinion as final, there is less reason for hesitation in stating that the conclusion I have come to is diametrically opposed to Mr. Bidwell's.

There seems to be no doubt that electricity will act on these very small drops of water in the same way as it acts on the drops in a jet of water. That its action is similar is easily proved by the following experiment with mist drops:—Take a small open vessel full of hot water—it is better to colour the water nearly black for convenience of observation—a cup of tea without cream does very well for the purpose. Place the cup on a table between the window and the observer. On now looking at the cup from such a position that no bright light is reflected from the surface of the liquid, there will be seen what looks like scum on the surface of the tea. That scum is, however, only a multitude of small mist-drops which have condensed out of the rising steam and have fallen on the surface of the liquid, where they are seen floating. If now we take a piece of brown paper, or any convenient material, and rub it slightly and hold it over the cup, the "scum" will disappear at once, and be replaced by other drops

* 'Phil. Mag.,' February 1890.

when the electrified body is removed. As in Lord Rayleigh's experiments, a very feeble electrification is sufficient to cause the absorption of the drops into the body of the liquid. It is therefore not because there is supposed to be any difference in the action of electricity on large and on very small drops that a different conclusion from Mr. Bidwell's has been arrived at, but because all the experiments to be described point to the conclusion that the dense form of condensation is not due to an increase in the size of the drops, but to an increase in the number, accompanied of course by a diminution in the size.

We may suppose the following to be something like the manner in which the electricity acts on the jet:—In a steam jet the rapid movements of the drops give rise to frequent collisions, and these result in the coalescence of many of the drops, so that each drop in ordinary condensation is made up of a number; but, when the jet is electrified, the electrification prevents the particles coming into contact, as they repel each other, and the consequence is, we have a greater number of particles in a dense and electrified jet than in an ordinary one.

Lord Rayleigh's experiments on the action of electricity on water jets support this view. He has shown that, in order to produce coalescence, the electrification must be very slight, and he also points out that the coalescence does not seem to be so much due to electrification as to a difference of electrification, which would appear to cause a discharge of electricity to take place between the drops, which ruptures the films, so causing contact. Further, he has shown that when the electrification is strong, and the conditions are such that the drops become electrified, the effect is diametrically the opposite, and instead of coalescence, the particles now scatter far more than the unelectrified drops. Now from the conditions of the experiments with electrified steam jets it is evident that the drops are electrified, and are in the same condition as the electrified scattering water jet. We are, therefore, entitled to expect that the electricity will prevent and not aid the coalescence of the small drops in the steam jet.

Other considerations also point to the increase in the density of the jet being due to an increase and not to a diminution in the number of drops. We know that if we blow steam into air, that the fewer dust nuclei there are in the air, the thinner is the condensation, and when the dust is nearly all out of the air, only a fine rain falls which can scarcely be detected by the unaided eye. Further, the evidence from condensation produced by expanding moist air points to the same conclusion, namely, that the more dust particles there are in the air, the denser is the condensation when cooled by expansion, and the purer the air is, the thinner is the cloud.* These experiments all

* 'Trans. Roy. Soc. Edinburgh,' vol. 30, Part I, p. 340.

point to the conclusion that the dense form of condensation is due to a large number of water drops, and the thinner form to a smaller number, though of greater individual size. The only condition under which it seems probable that the increase in number will not give rise to increase in density is when the particles are so small that they are unable to reflect waves of any colour of light. So far as has yet been observed this never happens. However slight the amount of expansion, the greater number of particles always gives the denser form of condensation.

The action of the electricity on the jet does not appear to be anything positive: it rather seems to prevent something which takes place under ordinary conditions. For instance, electricity has no effect in thickening the cloud of so-called steam rising from a hot and wet surface. The electrically driven current of air from a point when directed to the steaming surface has no effect whatever on the density of the condensation. Nor has electricity any effect on the steam rising from an open vessel. The small drops of water under these conditions move but slowly, and there is but little tendency for them to come into collision with each other; there are, therefore, few collisions for the electricity to prevent, and little or no thickening is produced by electrification under those conditions. Further on we shall have frequent opportunities of seeing that the dense form of condensation is the result of an increase in the number of particles, and that whatever gives rise to an increase in the number causes an increase in the density.

When the jet is electrified and becomes dense, it has been noticed by others that it emits at the same time a peculiar sound, and I find that whenever the jet becomes dense, from whatever cause, it begins "to speak." But when the density is due to electrification, the sound is, however, slightly different from the sound emitted when dense from any of the other causes. When dense from causes other than electrification, the sound is similar to that produced by the jet striking an obstruction; but when electrified, the sound is a combination of this sound with another due to the discharge of the electricity; and this second sound depends on the manner in which the electric discharge takes place. If the discharging point is not sharp, and the potential is just sufficient to cause discharge, then the discharge is not continuous, but takes place at short intervals; it becomes, in fact, a series of disruptive discharges, and gives rise to a fluttering noise. This fluttering sound is greatly increased if the point terminates in a small ball of about 1 mm. diameter, and it is entirely abolished if we use a very sharp point, or better, a flame. The discharge with either the very sharp point or the flame is perfectly continuous, and nothing but the slight hissing that accompanies all dense forms of condensation is heard when the jet is electrified.

It has generally been stated that the effect of the electrification is sudden and marked, that whenever the jet is electrified it at once becomes very dense. This, however, is due to the manner in which the jet has generally been electrified. Some degree of potential is necessary to produce a discharge from the point, and whenever the potential is high enough to cause this, it is sufficient to charge the drops high enough to give rise to a very dense condensation. But if we make the discharging point extremely fine, or assist the discharge by means of a flame, then, we may begin with electricity of a very low potential, and the increase in the density may be made to begin by almost imperceptible degrees and to increase slowly to the dense form by gradually increasing the potential.

We shall for the present leave the question of the effect of the ordinary and the dense forms of condensation on the light transmitted through them, as it will be better discussed after we have considered all the ways in which the jet may be made dense, and we shall now pass on to consider the second of those given in our list.

2. *An Increase in the Number of Dust Nuclei.*

It has been noticed by previous observers that a flame brought near the jet tended to make the condensation dense; but, in describing the experiments, a confusion has generally been made between the flame and the products of the combustion taking place in the flame. So far as I have been able to observe, flame has no effect on the density of the condensation. Neither a luminous flame nor the flame of a Bunsen burner has any effect so long as the products of combustion are kept away from the jet. But if the products are drawn into the jet, they have a very marked effect either in increasing or decreasing the density. If the flame is near and the gases are hot, they make the jet nearly invisible, but if the gases are cooled or are not in great quantity, then they make the jet as dense as if it were electrified. The simplest way of studying this latter effect is to bring the products of combustion to the jet by means of a metal tube 2 or 3 cm. in diameter and about ½ a metre long. A small flame about ½ cm. high, placed below the level of the jet, is used. One end of the tube is kept over the flame while the other can be brought near the nozzle. It will be found that when brought into that position the jet will at once become dense, and when it is removed it will return to its ordinary condition, and become dense again with every return of the impure gases.

The increase in density in this case is due to the greater number of dust particles in the gases offering a greater number of nuclei for condensation, and the result is a great increase in the number of water particles, and consequent thickening of the condensation, a

result which, as has already been stated, the author proved some years ago.

The change in the appearance of the jet when the products of combustion are brought to it, is exactly the same as that produced by electrification. The whole jet becomes dense, the condensed particles are visible nearly up to the nozzle, and the jet makes the same sound as when electrified by silent discharge, and further, electricity of the potential used does not make it any denser.

It seems probable that the very great number of dust particles in the products of combustion act in two ways: first, by supplying a great number of nuclei; and, second, as the number is greater the drops will be smaller, and, on account of their small size, they will have less independent motion, as they will be more guided by the gases than larger drops; there will, therefore, be fewer collisions, and not the same tendency to the diminution of numbers by the coalescence of a number of drops into one. It may be because of the small number of the collisions when the particles are small that electricity has little or no effect on the jet when it is dense from a large supply of nuclei. It is possible that some of the increased density produced by the products of combustion may be due to the slight electrification of gases from flames. But as the electrification from this source is very slight, its effects will be extremely feeble indeed when the dust particles are developed to the size of drops, so that the electricity from this source is not likely to have much effect.

3. *Cold or Low Temperature of the Air.*

We now come to the third cause of the dense form of condensation, namely, low temperature of the air. At first sight it may appear that the above statement contains an already well-known fact. But while in a certain sense this is so, yet there is one point of great importance which, so far as I am aware, has not previously been observed. If we were asked to state what is the effect of the temperature of the air on condensation of the jet, we, probably, would say that when the temperature of the air is high the condensation is very transparent, owing to there being less vapour condensed and to its rapid re-evaporation; and that when the temperature became lower and lower the jet gradually thickens as the temperature falls, owing to the greater amount of condensation caused by the colder air. Such a description is far from a full statement of the facts regarding the changes in appearance with the fall in temperature, and the explanation is correspondingly faulty. There is an influence at work in the condensing jet, which, though due to temperature, is of far more importance than the effect of the temperature on the amount of steam condensed.

When I first encountered this new influence it greatly puzzled me.

I had opened the window of the room where the experiments were being made, and when the fresh air came in, the jet began to behave itself in a most uncertain way. At one moment it was quite steady ordinary condensation, and the next it would conduct itself as if electrically excited. Even after the window was closed it continued to change from the ordinary to the dense form of condensation in a puzzling way. It was first thought that the outer air might be electrified, and tests were accordingly made to see if this were the case. These tests showed that if it were electrified it could be so but slightly, as it did not affect a gold leaf electroscope, which it would require to have done to have produced the increased density observed in the steam jet. Electricity as the solution of the difficulty had, therefore, to be abandoned. The only other influence I could think of as likely to cause the effect was some unknown effect of cold; I, therefore, took the metal tube which had been used in a previous experiment for conveying the products of combustion from the flame to the jet and cooled it. On now presenting one end of this cold tube to the jet it at once responded, and the condensation became as dense as if a flame had been at the other end of the tube, or as if the jet had been electrified.

This effect was all the more surprising since there was no great difference between the temperature of the air in the tube and that of the room, not more than 10° F. Some experiments were, therefore, made to find out the temperature at which this change takes place, and to see if it was as sudden as it appeared to be. The jet was supplied with air cooled in a pipe, which was surrounded with water for regulating the temperature of the air. The steam nozzle was placed just inside one end of the pipe and pointing outwards, so that the jet drew its supply of air out of the tube. No very satisfactory results were got with this apparatus. It may, however, be mentioned that when the air was cooled the jet somewhat suddenly became dense, and again became ordinary when the temperature was slightly raised; but with the apparatus it was difficult to say what the temperature of the air really was when the change took place.

Another method of studying the effect of temperature on the density was tried with fair success; the nozzle was fitted to the end of a horizontal pipe, the nozzle also being pointed horizontally. For this experiment a morning was selected when the temperature of the room was low. When the experiments began the temperature was 40° F. At this temperature the jet was always dense, and neither electrification nor the products of combustion increased its density. The room was now slowly heated, and the jet watched while the temperature rose. Up to a temperature of 46° no change took place, and the jet was not made denser by electricity nor by the products of combustion. But when the temperature rose to 47° the jet began to

show signs of clearing. The clearing did not, however, take place regularly; one moment the jet was dense and the next it was ordinary. These fluctuations would be due to the unequal temperature of the air coming to the jet. At one moment the air would be the air of the temperature of the room; the next it would be this air slightly heated by the metal pipe and nozzle. So that when the jet drew its supply of air horizontally its condensation was ordinary, and when the air currents in the room prevented this heated air from coming to the jet its condensation was dense.

A slight alteration was then made in the arrangement; the jet was now directed downward at the end of the horizontal pipe. By this means the air heated on the pipe and nozzle was prevented from mixing with the jet. The jet was directed at a small angle from the vertical to prevent the hot air and vapour of the jet rising to the nozzle. With this arrangement the following was the result: up to a temperature of 46° the condensation was dense, and neither electricity nor the products of combustion had any effect on the density; but when the temperature rose to about 47° electrification began to have just a perceptible effect in increasing the density. At about 48° the electricity had an easily observed effect, and the products of combustion also had a slight effect. At a temperature of 50° the jet had become decidedly thinner, and both electricity and the products of combustion had a decided effect in increasing its density. When the temperature rose to 55° the jet lost its dense appearance, and both electricity and the products of combustion had a very marked effect.

It might be thought that by observing a steam jet in the open air we could tell if the temperature of the air was above or below a certain point. This, however, can only be done in a very rough way, as the conditions are variable and not within our knowledge. We would require to know the pressure of the steam, and the degree to which the air was heated by the pipe. In a general way it may be stated that in the open air a steam jet looks dense if the temperature be below 50°, and ordinary if above 55°. But it is often difficult to say what is ordinary and what is dense condensation, unless the observations are made carefully and by examining how close to the nozzle the particles are visible. Of course if we could electrify the jet, or supply it with the products of combustion, we could tell whenever the temperature was over or under 47°.

The sudden alteration in the appearance of the jet when supplied with air at a temperature of 46° points to some change in the influences in action in the condensing jet. The great increase in density cannot be due to an increase in the amount of vapour condensed, as the fall in the temperature is slight. Further, it will be observed that the jet has ceased to be influenced by electricity, and by the products of combustion. The only explanation I could think of was, that at

the temperature of the mixed cold air and steam some alteration had taken place in the surface films of the water drops. The jet looked as if something came into action at that temperature which prevented the drops coalescing when they came into collision, or, what would amount to the same thing, that at high temperatures there was no tendency for the drops to recoil after impact, and that when the temperature fell this property made its appearance, and prevented contact in the same way as we have supposed the electrification does.

The simplest way of testing this explanation was to repeat Lord Rayleigh's experiment with water jets, but in place of cold water using hot. The result is, the experiment entirely confirms this explanation. So long as the water in the jet is above a certain temperature there is no scattering whatever, but perfect coalescence of the drops on contact. As a consequence the jet is not influenced in the slightest degree by the presence of an electrified body. It is only after the temperature falls below a certain point that the scattering commences, and electricity begins to have an influence.

This experiment shows that it is only when the drops are below a certain temperature that their surface films act in the way we are accustomed to observe at ordinary temperatures, that is, repel each other; and that when the temperature is high there is an entire change, and the surface films no longer repel, but coalescence of the drops takes place at each collision. It will be noticed that the point here is, not the appearance of any new influence with the low temperature, as the films are then in the condition with which we are acquainted; it is at the high temperature that the new condition comes into action, and the films lose the resisting action with which we are acquainted.

Now it seems extremely probable that the change in the appearance of the steam jet when the temperature of the air is lowered is due to the temperature of the jet falling to the temperature at which this repulsive action makes its appearance.

There is, however, an experimental link wanting to bind these two phenomena together, which I have desired to complete, but unfortunately experimental difficulties stop the way. The link wanting is some experimental proof that the jet gets dense at the same temperature that the water jet begins to scatter. On attempting to take the temperature of the jet difficulties presented themselves. If it is to be taken with a thermometer, where is it to be placed? A very slight change in the position of the bulb of a thermometer placed in the jet gives a different reading. It does not matter whether the change be made nearer or further from the centre of the jet, or nearer or further from the nozzle: in all cases a very slight change gives a considerable difference of temperature. It may, however, be stated that when the bulb was placed in the centre of the jet, and

near the nozzle, it showed a temperature of about 130°, but that figure can only be looked upon as a very rough approximation to the true temperature.

One or two attempts were, however, made to find the temperature at which water films cease to have any repulsive action. This was done by means of a small water jet; and it was found that above 155° there was no scattering. It was not till the temperature fell below that point that electrification had any effect. This was the temperature of the drops themselves, not of the supply for the jet; and it may not be quite accurate, as the drops tend to cool very quickly. Another method of finding this temperature was to observe the highest temperature at which the mist drops floated on water, in the experiment previously described. This method is not very satisfactory, on account of the difficulty of seeing the drops when the temperature is high, owing to the amount of condensed steam hanging over the water. It is also difficult to keep the surface of the water clean. The tests by this method gave a temperature considerably higher than that given by the water jet. Neither of these methods, however, promises to give satisfactory information on this point; but, if it were desired, the effect of temperature on the contact of films could be studied in a more accurate way.

It is difficult to imagine any sudden change in the action of the films at or about the temperatures indicated. There is no corresponding change, so far as I am aware, in the surface tension. We might picture to ourselves the change to be brought about by the alteration which takes place in the intervening gases. When the drops are cold, the bounding surfaces are water and air with very little vapour in it. And perhaps we may be permitted to assume that the surface-film has a layer of air condensed on it, and it may be this condensed layer of air which prevents contact when the drops come into collision. But when the temperature is high, the conditions are changed. The bounding surfaces are now water and air with a large amount of vapour in it, and this vapour may play an important part in bringing about the contact, by the violent interchange of water molecules taking place at the surfaces of the films, and weakening the condensed films of air. If this explanation be correct, then there is really no sudden change in the action of the films, and the repulsion is a gradually increasing one with fall of temperature. Though the somewhat sudden change in the appearance of this jet might seem to indicate a sudden change in the action of the films, yet the change may be really a slowly increasing one, and the sudden change in the appearance of the jet may be due to the repulsion rising to such an amount that the very small particles are prevented from coalescing. If the relative temperatures given for the coalescence of water drops and mist drops be correct; then the

gradual rise in the repulsion with fall in temperature may be the explanation of why the drops in a water jet coalesce at a lower temperature than the mist drops on the surface of water. The water may require to be cooled to a lower temperature before the repulsion is sufficient to prevent the heavier drops from coalescing, while the less repulsion at the higher temperature may be sufficient to prevent the lighter mist drops from coming into contact. The same explanation helps to account for the increased density produced by increasing the dust particles, a less repulsion being sufficient to protect the excessively small drops.

The explanations we have here offered of the action of electricity and low temperature are in complete agreement. In ordinary condensation when the temperature of the air is high there is no surface repulsion, owing to the high temperature in the jet, and many of the particles coalesce on collision with each other; but, when the drops are electrified, their mutual repulsions prevent contact, and the result is a large increase in the number of drops and a dense form of condensation. On the other hand, when the temperature is lowered, surface film repulsion comes into action, contact is prevented, and the drops do not coalesce on collision, and the result is exactly the same as if they were electrified.

In these remarks no reference has been made to the effect of the dryness of the air on the density of the condensation. It seems probable that the relative humidity of the air will have a less influence on the density than on the duration of the jet, that is, the length of time the drops take to evaporate.

4. *High Pressure of the Steam.*

The fourth cause of the dense form of condensation is high pressure of the steam. If the temperature be below 46° the condensation is dense at all pressures, but as the temperature rises, the condensation ceases to be dense if the pressure of the steam be low. But if we now raise the pressure, the jet again becomes dense, and the higher the temperature of the air the higher the pressure must be raised to produce the dense form of condensation. The action of the high pressure in producing the dense condensation is more complex than any of the previous causes. It acts, first, by the more rapid movements of the jet mixing a larger amount of air with the steam, by which means a greater number of dust nuclei are taken into the jet; and, second, a lower temperature is also produced, which probably brings the temperature of the drops low enough for the repulsive action of the films to come into play. But in addition to the effects of a greater amount of air being mixed with the steam, a third action here comes into play. Owing to the violent rush of steam, the con-

densation takes place more rapidly; and it has been found that, the more rapidly the condensation is effected, the greater is the number of particles formed. If the condensation take place slowly, a much less number of nuclei are sufficient to relieve the supersaturation, as there is time for the movements of the water molecules to take place; but if the rate of condensation be forced, then the tension of supersaturation compels a great many more dust particles to become centres of condensation. The result of this is, that with two samples of the same mixture of air and steam, if one of them be condensed slowly, the clouding is thin, while if the other be condensed quickly, it is thick. This action will come into play in the steam issuing at high pressure, when the steam is rapidly expanded, cooled, and then mixed with cold air.

The increased density produced by increase of pressure also takes place somewhat suddenly, though not quite so suddenly as when the density is produced by the other causes. The jet first gradually thickens as the pressure rises, then a stage is arrived at when it somewhat suddenly becomes dense. When this last stage is arrived at, neither electrification nor the products of combustion cause any increase in the density. The first thickening is probably the result of the quickening of the condensation and increase in the number of dust nuclei; and the sudden increase in density is probably due to the temperature falling low enough for the films of the drops to have a repulsive action, sufficient to prevent them coalescing.

5. *Rough Nozzles and Obstructions in front of Jets.*

If we use a nozzle of irregular form, or having roughened edges, it is found that it gives a dense condensation at a lower pressure than a nozzle of circular section with smooth bore and thin even edges. This is owing to the irregularities in the nozzle producing eddies in the jet, and mixing a greater amount of air with the steam, so cooling it more and supplying it with a greater number of nuclei. It, in fact, acts in the same direction as increase of pressure, and aids pressure in producing its results with a less velocity of steam.

An obstruction in front of the jet acts in a similar manner, if we have a jet of steam of such a pressure that at the temperature of the air it gives only the ordinary form of condensation. If now we place an obstruction in front of the jet so as to produce eddies, the condensation at once becomes dense. Wind has also a somewhat similar effect. The reason of the increased density in these cases is the same as for the jets issuing from irregular nozzles. They all assist the pressure in intensifying the density of the condensation, by lowering the temperature of the jet, increasing the number of nuclei, and quickening the rate of condensation.

The Seat of the Sensitiveness of the Jet.

The seat of maximum sensitiveness to all influences tending to change the condensation from ordinary to dense is near the origin of the jet close to the nozzle. The different influences, however, affect the jet to different distances from its origin. The most limited in the range of its action is cold, which only produces the dense condensation when it acts near the nozzle, whereas some of the other influences have some effect, though a gradually decreasing one, to a distance of 2 or 3 cm. from the nozzle.

The following experiment illustrates the limited range of the action of cold:—A piece of ice about 2 cm. thick was selected, and a small hole bored through it. The ice was then held so that the steam jet passed through the opening. While the ice was held at a distance of 1 cm. from the nozzle, almost no effect was produced, though much cold air from the ice was mixing with the jet. But when the ice was brought nearer the origin of the jet, so that the nozzle almost entered the plane of the ice, the dense condensation immediately appeared.

The range of sensitiveness of the jet to change of condensation by obstructions is also very limited. It is only when the obstruction acts near the nozzle that its effect is great. For instance, the blade of a knife resting on the nozzle, with its back or edge pointing in the direction of the jet, and depressed so as to deflect the jet slightly, causes the jet to become very dense. But if the knife acts on the jet at a distance of only 1 cm. from the nozzle, very little increase in density is produced.

The range of action of electricity is much greater than that of cold or obstructions. If we screen the nozzle and the part of the jet near it from electrification, it will be found that at a distance of 3 or 4 cm. a slight increase in density can be produced with the electrification used in these experiments.

The action of the products of combustion has a range similar to that of electricity. If the products are supplied to the jet at a distance of 3 or 4 cm. from the nozzle, a slight increase in thickness can be detected where the impure gases meet the jet; but the effect is very slight compared with that produced when the gases are taken in at the origin of the jet.

The limited range of the action of cold is quite what might be expected. Near the nozzle the temperature of the jet is high, and there the drops have no repulsive action; but at a short distance from the nozzle the temperature is low enough to allow this repulsion to come into action, and the consequence is that any further cooling after the temperature is below a certain point produces little or no effect. It is only when the temperature is above this point that

the cooling has any influence. The same explanation holds good for the limited range of the action of obstructions in front of the jet.

At a distance of a few centimetres from the nozzle new drops seem still to be forming, as the density of the condensation is slightly increased by increasing the supply of nuclei at that distance. The drops seem also occasionally to coalesce at a distance of 3 or 4 cm. from the nozzle, as electricity slightly increases the density of the condensation even at that distance.

Part II.

Colour Phenomena connected with Cloudy Condensation.

In the following remarks it is not intended to discuss the many colour phenomena which are known to be connected with cloudy condensation. Attention will be confined principally to some new phenomena, the experimental illustration of which has been developed in the present investigation.

Before describing these experiments, it may be as well to refer to some changes which take place in the constitution of cloudy condensation, both while it is forming and after it has been developed, as it will be necessary for us to keep certain points in view while discussing the colour phenomena. There are two points to which special reference is required. These are, first, the manner in which the appearance of the condensation is affected by the greater or less degree of supersaturation, that is, by the rate at which the condensation is made to take place; and, second, the changes which take place in the appearance of this cloudy condensation after the tendency for the vapour to deposit has stopped.

These two points may be best discussed by taking the second first. Suppose we blow some steam into the air inside a glass vessel, and leave it undisturbed; if we examine the cloudy condensation after a time, we shall find that a considerable change has taken place in its appearance. The change is due to two causes: part is due to the gradual descent of the particles by which a clear space is formed in the upper part of the vessel. But it will also be observed that the clouding in the lower part is much thinner than it was at first. Probably part of this thinning is due to some of the particles having fallen to the bottom of the vessel, but this is not the principal cause of the change. The thinning is due mainly to a reduction in the number of particles in the air, by the smaller particles gradually becoming absorbed by the larger ones. This is caused by the vapour-pressure at the surface of small particles being greater than at the surface of larger ones, with the result that the smaller particles evaporate in air of the same humidity in which the larger ones are *condensing* vapour.

We are now in a position to understand our first point, namely, why the degree of supersaturation by which the condensation is produced should have an effect on the appearance of the condensed vapour. For the study of this point the condensation produced by expansion is the most convenient, as it is more under our control than the condensation in steam jets. Suppose we take a glass flask connected with an air-pump. If we wet the inside of the flask and then fill it with unfiltered air, the slightest expansion of the moist air by the pump will cause condensation to take place. But the density of the condensation which can be produced by any degree of expansion will depend on the rate at which the expansion is made. If the expansion be made very slowly, the clouding is very thin; but, if it be made rapidly, it is very thick. If the expansion be done slowly, the amount of supersaturation is only slight, and only the largest dust particles come into action as active centres of condensation; and after a particle of dust has once become a nucleus, it has then, in virtue of its size, an advantage over the particles which have not begun to have vapour condensed on them. The result of this is that, so long as the degree of supersaturation is very slight, these large particles relieve the tension, and if by any chance other dust particles become active, any reduction in the rate of condensation allows the large particles, after they have relieved the tension, to rob the small ones of their burden of water, so that a slow rate of condensation always produces a small number of drops and a thin form of clouding.

But now suppose you cause the expansion to be made rapidly; the supersaturation then becomes much greater, as there is not time for the water molecules to select a resting place, and the small number of large dust particles cannot relieve the tension, and the result is a much greater number of nuclei are forced into action. And all these nuclei continue to grow so long as the supersaturation is kept up, but the larger ones grow most. After the tendency to condense has begun to diminish, those particles which have accumulated least are the first to feel the change, and cease to grow, while the larger ones are still accumulating. But after the tendency to condensation has ceased altogether, the changes in the clouded air are not at an end. The smaller drops begin to lose their accumulated moisture, while the larger ones are still growing—growing at the expense of the gradually diminishing smaller ones. This process goes on till most of the small ones have lost all their burden of water, which has been absorbed by the overgrown larger ones; and in the end a comparatively small number of drops have absorbed the moisture which was previously distributed over a vast number of particles. The larger particles have, so to speak, eaten up the smaller ones. How like the above looks to a page in the "struggle for existence" in the animal or vegetable world!

Colour Phenomena in Steam Jets.

Steam escaping into the atmosphere has been observed on a few occasions to have the power of absorbing certain of the rays of light, and causing the sun, when seen through it, to look "blue" or "green." Principal Forbes observed colours in the steam escaping from a safety valve. Mr. Lockyer* states that, when on Windermere, he saw the sun of a vivid green, through the steam of a little paddle-boat. I believe a few others have seen this phenomenon under similar conditions, but so far as I am aware no one has followed out the suggestion, and investigated the manner in which the colour is produced.

Mr. Bidwell, in his experiments on the electrification of steam jets, studied the action of the jet on light, by casting the shadow of the jet on a white screen, using for illumination the lime light. He found that the shadow of the ordinary jet—that is, the light transmitted by the jet—was nearly colourless, but that when it was electrified the shadow became of a dark orange-brown colour.

The colour of the "green sun" seen through steam has been attributed to the absorption of both ends of the spectrum by the aqueous vapour. This explanation is obviously not the correct one, as it will be found that a moderate length of steam has no perceptible selective absorption. Through a length of even one metre of steam, white objects are not coloured, and we shall presently see that the colouring depends not on the vapour, but on some action of the small drops of water in the condensing steam.

For the purpose of studying the colour phenomena of steam jets I have found it to be a great advantage to surround the jet by solid walls. When a jet condenses under ordinary conditions, the constitution of the jet rapidly changes in its passage away from the nozzle, owing to the air mixing with it; and it has been found that by enclosing the jet in a tube, after a certain amount of air has been mixed with the steam, that the conditions can be kept fairly constant for some length of time, and the colour phenomena taking place can, therefore, be more easily studied under these conditions. The tube used for this purpose need not be of any special size. For a jet from a nozzle of 1 mm. bore a tube of 7 or 8 cm. diameter, and about half a metre long, does very well, but a smaller and shorter tube may be used. With the larger size of tube it may be necessary to check the current through it. This is best done by placing a piece of glass near the exit end of the tube, the opening between the glass and the end of the tube being regulated to the required amount by observation. When a small jet of steam is used with the large tube

* 'Nature,' vol. 18, p. 155.

open at both ends too much air is drawn in, and the effect is much the same as if no tube were used. The end of the tube has, therefore, to be closed to a certain extent, to produce the colour phenomena. But when high pressed steam is used, no check on the circulation through the tube is necessary. The steam nozzle should be placed outside the tube and a little to one side, so that the eye can be brought into a line with the axis of the tube and a clear field of view obtained while the jet plays into the open end of the tube. This is an experiment which well repays the trouble of making it. When the amount of steam, dust, and other conditions are properly proportioned, the colours seen are very beautiful. With ordinary condensation the colour varies from a fine green to lovely blues of different depths. The pale blues equal any sky blue, while the deeper blues are finer than the dark blues seen in the sky, as they have none of the cold hardness of the dark sky blues, but have a peculiar softness and fulness of colour.

Suppose now the tube is fitted up pointing to a clouded sky, or other source of light, and that the steam jet, under slight pressure, is blowing through it. If the exit end of the tube be open, we shall see very little colour, and what is seen is only near the origin of the jet. If now we partially close the end of the tube with the glass plate, to prevent the jet drawing in so much air, we shall find that colour begins to appear, and that when the plate is properly adjusted the tube looks as if filled with a transparent coloured gas. The first decided colour to appear is generally green, though I think I have frequently seen a pale crimson before the green was visible. If the circulation be checked still further, the colour will change to blue of a greater or less depth according to the conditions.

The above are the effects which may be looked for when the condensation of the jet is ordinary; but suppose it be now caused to change to the dense form, then the colour seen through the tube also changes. If, when the jet is condensing in the ordinary way, and the transmitted light is green, we cause the condensation to change to dense, then the colour also changes and becomes deep blue, or, if the ordinary condensation gave blue, the colour changes, when the jet is dense, to a dark yellowish-brown. But between the blue and the yellow there is always an intermediate stage when all colour disappears and the light is simply very much darkened. The most common effect of the change of the condensation from ordinary to dense is for the transmitted light to change from blue to a yellowish colour, and it does not matter how the change in the condensation is effected; the colour always changes in the same way. We can, therefore, cause the colour in the tube to change by electrifying the jet, by a supply of cold air, by a supply of the products of combustion, by increasing the pressure of the steam, and by placing an ob-

struction in front of the nozzle. When any of these, either separately or combined, comes into action, the change is always in the same direction, and if the colour was blue, it changes to yellow.

It may be as well to note here that the yellows produced by most dense forms of condensation are far from fine, and cannot be compared with the blues. The yellows are not at all unlike the colours occasionally seen through smoke, or in a thunder cloud. Though this is the case with the dense condensation produced by most of the causes, yet a very fine yellow is obtained when high-pressed steam is used.

It has been suggested that, because an electrified jet causes the light transmitted through it to be coloured of a dark yellow-brown, and as the colour seen in thunder clouds is similar, that, therefore, the lurid colour of thunder clouds is due to the electrification. From what is stated above, it will be seen that electricity is only one of a number of influences which can change the condensation of the steam jet and make the light transmitted through it of a yellow-brown colour. Further, there is no evidence to show that electricity has any influence of this nature on the form of condensation taking place in clouds, and we are hardly entitled to expect it to have any such influence, as the conditions under which the steam condenses in a jet are very different from those under which condensation takes place in clouds; and we have seen that electricity has no effect on the nature of the condensation when it takes place in a mixture of hot moist air and cold air. There is still another fact which points to the same conclusion. If, in the steam jet, the proportions of dust, pressure, &c., are such as to give an earlier stage than the blue, suppose the transmitted light be green, then the electrification may not change it to yellow, but may only make it blue. At present, it is therefore very doubtful whether the electricity in a thunder cloud has anything to do with its colour.

Colours observed in Cloudy Condensation produced by Expansion.

Though previous experiments had made me well acquainted with certain colour phenomena, seen when cloudy condensation is produced by the expansion of moist air in a receiver, yet I had never observed any colours in the light transmitted directly through the clouded air, such as are seen in the jet of steam when enclosed in a tube. It seemed extremely probable that the reason for this would be, that when the condensation is produced by expansion, the process is slow, and the particles will, therefore, be too few to produce any colour effects. In a steam jet, the expansion, cooling, and condensation take place very rapidly, and for that reason the number of water particles formed is very great. An experiment was therefore

arranged, in which the air could be very rapidly expanded, so as to produce a high degree of supersaturation, which it was hoped would cause a great number of dust nuclei to become active. To test this idea, all that was necessary was that the receiver used for holding the moist air should be much smaller than usual in comparison with the capacity of the pump, and that the light be transmitted through some length of air. The plan adopted was to use an air-pump of ordinary dimensions, and for a receiver, a metal tube closed with glass ends. The first apparatus prepared for this experiment was found to give satisfactory results, and the alterations since made have not been of any great advantage.

The apparatus consists of a brass tube 2·3 cm. diameter and about half a metre long. It is provided with glass ends, fitted on air-tight, and is provided with a branch pipe at each end. One of these branch pipes is connected with an air-pump, and the other has a stopcock fixed to it. This stopcock is connected with a pipe for bringing to the tube the air to be experimented with. If the tube be mounted horizontally, the particles rapidly fall and the phenomenon is visible for only a short time. The tube is, therefore, best mounted vertically, and with a mirror placed at the lower end of the tube to reflect the sky or other source of light up through the tube to the eye of the observer.

The air-pump used is a single cylinder instrument of 3·17 cm. diameter and 19·3 cm. stroke, so that its capacity is about three-quarters that of the tube receiver. If we take the instrument outside the house and make one or two strokes of the pump to fill the receiver with air of the place, then close the stopcock, and make a rapid stroke with the pump, little effect is produced on the light transmitted through the tube. But if we take the instrument into a room where gas has been burning, so that the air is full of dust particles, and repeat the experiment, very beautiful colours are seen on looking through the tube when the air is expanded. Or, better still, if we collect the gases rising from a small flame and draw them into the tube, the result is a display of an exceedingly lovely series of colours, full, deep, and soft, in some respects reminding one of polarisation colours. As in the steam jet, the blues are the finest, and the tube looks, at times, as if filled with a solution of Prussian blue. The colours produced in this way are more uniform and equal in all parts than those seen in the steam jet, unless when the jet is very carefully adjusted; the yellows are also much finer, and the colours more varied than those seen in the steam jet.

There is, however, one most disappointing thing connected with these colours produced by expansion: they are very fleeting. Their full beauty lasts but a second or two, and they soon fade away, the colour growing dimmer and feebler every moment. This is owing to

the differentiation which takes place in the particles forming the cloudy condensation. As has been already explained, the small drops rapidly diminish in size while the large ones increase, and as in these experiments the drops are very close to each other, these changes take place the more rapidly. These changes are also taking place in the steam jet, but, owing to the constant supply of new drops, the older ones are swept away before the change is observed. The following experiment will, however, show that these changes are taking place in the steam jet also. If, while the jet is condensing dense and the transmitted light is yellow, we imprison some of the jet by closing both ends of the tube, we shall find in an extremely short time that the colour will change to blue, after which it will fade as the drops increase still further in size, and fall. In this experiment we have a proof of the statement that when the jet is electrified the drops are smaller than when not electrified, and not larger, as has been supposed; as this experiment shows, if we begin with drops transmitting yellow light, that as the drops diminish in numbers and increase in size, the transmitted light changes to blue.

The conditions of the experiments for producing colour by cloudy condensation, produced by expansion, have been varied in a number of ways. After the air has been cooled by the expansion, the layer of cloudy air in contact with the walls of the tube rapidly acquires heat from the metal, and the rise in temperature quickly evaporates the cloudy particles and causes a clear space all round next the walls, so limiting the colour to the centre of the tube. The receiver was therefore increased in diameter to get rid of the disturbing effects of the heating of the air on the walls of the tube, so as to have a larger mass of air beyond this influence; but no decided advantage has been obtained. It was afterwards found that the difficulty of studying these colour effects in small tubes can be easily overcome by wetting the inside of the tube. With this precaution the air next the walls is kept saturated and the temperature of the walls is lowered by the heat given off to evaporate the water, with the result that the colour is the same all over the field and close up to the walls.

Large tubes might be used for showing these colour phenomena to an audience, a parallel beam of light being sent through them, which would become coloured when the dusty air in them was expanded. One large tube tried has a diameter of 7 cm., and is 50 cm. long. With a receiver of that capacity it would be hopeless to attempt to produce any colour effects with an ordinary air-pump alone; a vacuum receiver has, therefore, been added to the apparatus. This receiver is made of metal; it is 15 cm. diameter and 60 cm. long, with round ends. There are two tubes attached to it, one for connecting it with the air-pump, and the other is provided with a stopcock, to which a tube is attached,

by which it is connected with the experimental receiver. The stopcock is closed, and the pump worked till most of the air is taken out of the receiver; and when it is desired to expand the air in the experimental tube the stopcock is opened, when a violent rush of air takes place, and the pressure is rapidly lowered in the experimental receiver, and a dense colour-producing form of clouding is produced.

The Conditions causing the Different Colours.

For studying the conditions which give rise to the different colours seen in these tubes, the air-pump and the small tube will be found to be the most suitable. Supposing these to be fitted up, the following will show how the colours change with the conditions:—

First, the effect of the degree of saturation of the air. If the air be dry the colours are not good, and some degree of expansion requires to be made before any effect whatever is produced on the air; and when the colours do appear, it is only in the centre of the tube that they are seen, the space all round next the walls being free from condensed particles. As the humidity is increased, this unclouded space near the sides of the tube gradually diminishes. The colours are, therefore, best studied when the air is saturated and the inside of the tube wet. When this is done the colours extend to the walls, and completely fill the tube.

Second, the effect of the number of dust particles in the air. If we use ordinary outside air, the colours are very faint or invisible. Suppose some slight colour is visible, then it will be found that a very slight expansion, say one-fifth of a stroke of the pump, will give a pale blue, and if the expansion be increased the colour will change. If, now, we use air from a room where gas is burning, and fill the tube with it, we shall now, on expansion, get a much deeper blue, and it will be observed that a greater expansion must now be made to get the best blue, and before the colour begins to change. If we alter the conditions still further, and fill the tube with air in which is mixed a good deal of the products of combustion, we shall find that the condensation is now so dense that we can scarcely see through the tube; but it will be noticed that the colour is a very deep blue, and that a full stroke of the pump was necessary to produce this deep blue, but in this case no change of colour was produced with that large degree of expansion.

These experiments show that, with few dust particles, a slight expansion will produce the best blue, and that, as the number of particles increases, the amount of expansion necessary to produce the best blue also requires to be increased, the depth of colour increasing with the increase in the number of dust particles. The explanation of the differences here is very simple. With few particles a very

slight expansion will deposit enough moisture to make the small number of drops of the size sufficient to give the best blue colour; but, as the number of particles is increased, more moisture must be deposited before the increased number of drops are made large enough to give a full blue; hence with a larger number of particles a greater expansion is necessary to produce this effect.

Third, the effect of the size of the condensed particles. As has been stated, a slight expansion produces a blue colour if the number of particles be small, and if the expansion be increased after the blue is produced, the colour changes; and we shall now describe the successive colours which appear as the degree of expansion is increased, that is, as the size of the water particles is increased. When the expansion begins, blue is the first distinct colour to appear, but very pale yellow and slightly reddish colours have been noticed before the expansion was sufficient to produce the blue. These reddish colours can be seen very distinctly when we use an excessively great number of particles, and they are best seen with gas light. These reddish colours imperceptibly change into blue as the expansion is increased, and the blue in turn changes by minute degrees into green with further expansion, and the green in turn changes to yellow; then a brownish colour appears, which changes to a somewhat mixed purple; then the blue returns again, to be followed by green and yellow, as the expansion is still further increased. It is not easy to get this sequence of colours carried so far. Sometimes one stroke of the pump only carries the colour on to yellow; sometimes it may go to the second blue or green, but less frequently to the second yellow. The final colour depends on the number of particles present. It is necessary to have a good many drops, so that the colour may be distinct, and yet not too many, or the expansion may not be sufficient to grow the particles large enough to give the second series of colours. It is found that a high expansion, produced by two or more strokes of the pump, does not give satisfactory results.

We have seen that by increasing the number of dust particles the depth of colour was increased; it therefore seemed possible that these colour phenomena might be made visible in even a short column of air, and that they might be shown by means of ordinary glass flasks. The following experiment was, therefore, arranged:—A flask, about 18 cm. diameter, was fitted with an india-rubber stopper, through which passed two tubes. One tube was connected with the metal vacuum receiver already described, the other had a stopcock attached to it. The stopcock was connected with a long metal pipe, which led to a wide tube placed over a small flame. Air charged with the products of combustion was drawn into the flask through this pipe; when sufficient impure air was drawn into the flask, the stopcock

was closed; when the air in the flask was now suddenly expanded, it looked as if it had been filled with a transparent blue gas. The colour, when held against a white cloud, was almost exactly the same as that of the blue sky. The colour in the flask faded rapidly, as in the experiments with the tube. The particles being very closely packed in most of these experiments, the subsequent change is all the more rapid.

Effect of Temperature.

To observe the effect of temperature on these colour phenomena, another tube was prepared, with glass ends, and jacketed, so that the air in it might be heated or cooled to any desired temperature. The result was very much what might have been expected: at the different temperatures all the colours made their appearance in the usual order, but there was a considerable difference in the amount of expansion required to produce a given colour with change of temperature. At a high temperature each of the colours appeared with a less expansion than when the temperature was low. In making these tests the number of dust particles in the air must be kept as constant as possible. For this purpose windows and doors should be kept closed for some time before beginning, and the experiments should be repeated without change of conditions. When the air was cooled to about 35°, it took two strokes of the pump to develop a full blue, and three strokes made it only green. At a temperature a little over 50°, two strokes made it green, while if the air was heated to about 80°, two strokes sent it past blue and green and on to yellow, and less than one stroke made it full blue. These differences are due to more vapour being present and being condensed, with the same amount of expansion, when the air is hot than when it is cold. It should be stated that in all cases the air was saturated, the inside of the tube being wet.

The tube was also cooled down to 6° F., but no difference was observed in the nature of the phenomena. The particles at that temperature seemed to be still in the liquid form.

Light Transmitted.

The light transmitted directly through the cloudy condensation has been examined by means of a small spectroscope. One of the tubes was mounted vertically, and a mirror placed at the lower end; the spectroscope was temporarily mounted over the upper end of the tube; a small mirror was placed between the spectroscope and the glass end of the tube. This small mirror covered half the field of the spectroscope, and reflected light from the same source as that reflected by the mirror at the lower end of the tube, so that one-half of the field

gave the spectrum of the light, and the other half the light after passing through the cloudy condensation. The conditions in the experiment are too fleeting for satisfactory observation. The only thing noticed was a darkening of the whole spectrum, with a greater absorption at certain points than at others. When the light was blue, in addition to the general reduction in brightness, the red end was more reduced than any other part, and there was also a very marked shortening of the spectrum at this end. When the colour was yellow, the reverse was the case. The blue was almost entirely cut out, while the yellow was far the brightest part of the spectrum.

An examination has also been made of the diffraction colours as seen in the halos surrounding bright lights. The most convenient way tried of observing these colours was to use an ordinary glass flask of 18 cm. diameter, connected with the metal vacuum receiver, as already described. For the source of light gas may be used, but a better result is obtained with the light of the sky. In order to observe these colours easily, the window should be closed, all but a narrow vertical strip; and it improves matters to have all surfaces on each side of the opening painted black. When the air in the flask is expanded, the vertical bands of diffraction colours are distinctly seen on each side of the bright light. If now we keep the amount of dust in the air constant during the experiments, we shall find, that on opening the stopcock to the vacuum receiver very slowly, we will get the usual cloudy condensation, and that the diffraction colours will be quite distinct. But if we repeat the experiment, and this time open the stopcock very suddenly, so as to cause a rapid expansion, the colours will be found to be very much improved, being far more brilliant. This is due partly to the greater number of particles engaged in producing the effect, but chiefly to the much more equal size of the particles when they are suddenly developed than when slowly grown.

It is found that we must not have too many particles present, or the diffraction colours will not be good; their size does not seem to be great enough to produce the phenomena. If, for instance, in place of using the air of the room, we take into the flask air coming from a small flame, the colour phenomena in the flask all change: when there were few particles the light transmitted directly through them has so little colour it is not noticed, while the diffraction colours are fine; but with many particles the direct light becomes coloured, while the diffraction colours are softened and have lost much of their brilliancy. When the particles are sufficiently numerous to cause the directly transmitted light to be of a thin blue, the diffraction colour next the blue light is nearly the complementary yellow, and this yellow light extends to near the limits of the flask. If more particles be added, the colour of the transmitted light becomes deeper blue, but it

is difficult now to say what the diffraction colours are. The convection currents in the flask now make themselves visible; the air on each side of the blue direct light is suffused with a variety of colours, not now in regular vertical bands, but irregularly distributed and in movement through the flask.

Cause of the Colour.

These experiments show that the colour produced by the small drops of water depends on the size of the drops, and the depth of colour on their number. But it is not so easy to follow the manner in which the drops produce the colour. If we take the simplest case, we can easily see how part at least of the colour is produced. In the steam jet condensing dense, and colouring the transmitted light yellow, part of the effect is no doubt due to some of the particles in that form of condensation being so small that they reflect and scatter the shorter waves of light, while they allow the longer ones to pass through. The colour in this case is partly caused in the same way as the yellow produced by small particles suspended in liquids, as in Brücke's experiment with mastic, or as when silver chloride is formed from a solution of the nitrate. The light reflected by the liquids in these experiments is of a bluish tint, complementary to the yellow light transmitted by them, and this blue light is polarised. It has been found that when the steam jet is of a good yellow by transmitted light, it reflects a good deal of a bluish light; and further, this blue light is polarised in the same way as the light from the small particles in the experiments with liquids.

While this explanation helps us so far to understand the manner in which the yellow light is produced in steam jets, yet it fails to explain the succession of colours seen in the expansion experiments, where blue first appears, then green and yellow; and when the expansion is still further increased, the blue again returns to give place to a second green and yellow. The most probable explanation of these colour phenomena is that they are produced in the same way as the colours in plates, somewhat after the manner Newton thought the colour of the sky was produced. The order of succession of the colours in thin plates is the same as in these condensation phenomena. As no white follows the first blue, it seems probable that the first spectrum, or order of colours, is not observed; that the two generally seen are the second and third.

Some experiments were made with a glass tube receiver, in place of the metal one, to see if there were any coloured light reflected in these expansion experiments of the same kind as is seen under certain conditions in the steam jet; but no such colours have been observed. It is possible they may be present; but, owing to the great amount of

white light reflected by the larger particles, any coloured reflected light that may be present is masked.

Green Sun.

On a few occasions the sun has been observed to be of a decidedly greenish colour, while on other occasions it has appeared blue. The experiments which have been described in this paper seem to offer an explanation of this phenomenon. For a number of days in the beginning of September, 1883, the sun was seen of a decidedly blue or green colour in India, Trinidad, and other places. Most of the observers who have written on the subject have linked this phenomenon with the eruption of Krakatao, which took place just before the days on which the green or blue sun was seen. From the light thrown on the subject by these experiments, we see that an eruption, such as that of Krakatao, would throw into the atmosphere a supply of the very materials necessary for producing a green sun by means of small drops of water, as it would send into the atmosphere an immense quantity of aqueous vapour and an enormous amount of fine dust—a combination the most favourable possible for producing a great number of minute drops of water.

Professor C. Michie Smith observed the green sun in India, and he says: "The main features of the spectrum taken *on* the sun when green were—

"1. A very strong general absorption in the red end.

"2. A great development of the rain-bands and of all other lines that are ascribed to the presence of water-vapour in the atmosphere."*

It is evident, therefore, that one of the materials necessary for producing this peculiar absorption by means of water-drops was present in an unusual amount in the atmosphere at the time; and it also appears that a fine form of condensation had taken place, as Mr. W. R. Mauley states† that there was at the time a sort of haze all over the sky, and from the letters of different observers this haze seems always to have accompanied the green sun.

One almost wonders that a blue or green sun is not oftener seen, as there are often present all the materials necessary for producing these colours in the atmosphere. On a few occasions I have observed the sun to be of a silvery whiteness, when the vapour in the upper air was beginning to condense, and the sky was covered with a thin, filmy cloud. It is, however, possible that this slightly bluish tint may have been due to the sun being seen more in its natural colour than usual, that is, made much less yellow by our atmosphere than it gene-

* 'Nature,' vol. 30, p. 347.

† *Ibid.*, vol. 28, p. 576.

rally is. There seems to be something preventing the dust and the vapour in our atmosphere acting under ordinary conditions in such a way as to colour the sun blue or green. Perhaps it may be the tendency the particles have to differentiation. This tendency, we have seen from the experiments, rapidly destroys all colour effects, and from this we might suppose it would be impossible that the colours, if produced by water drops, could remain in nature visible for so long a time as they did. But it must be remembered that the particles in the experimental vessels are extremely close together, and the vapour exchanges can therefore take place quickly. If, however, the drops were widely separated, the exchanges would take place slowly. For instance, if the drops in 1 cm. were separated so as to form a column 1 mile long, with a section of 1 sq. cm., we should have the same amount of colour in 17 miles that we had in the 17 cm. of air in the flask, and the particles would be so far apart that differentiation would then take place extremely slowly. But further, if the supply of dust and vapour were constantly kept up by the volcano, the colour phenomena would continue for the same reason that they continue in a steam jet, namely, by the drops being constantly renewed.

A New Instrument for Testing the Amount of Dust in the Air.

As this investigation progressed it became evident that these colour phenomena placed in our hands an easy and simple way of estimating, in a rough way, the number of dust particles in the atmosphere of our rooms, which might be useful for sanitary purposes. An instrument was therefore constructed to see how far the idea could be practically carried out. This new instrument we intend to call a *Koniscope.* In its present form this instrument consists of an air-pump and a metal tube with glass ends, which we shall call the test-tube. The capacity of the pump should be from half to three-quarters the capacity of the test-tube. Near one end of the test-tube is a passage by which it communicates with the air-pump, and near the other end is attached a stopcock for admitting the air to be tested. The test-tube and air-pump may be attached parallel to each other, and held vertically when observing. If this arrangement be adopted, a mirror must be attached to the lower end of the tube. In practice it is found to be more convenient to omit the mirror, and observe with the tube in any position, simply directing it to any suitable light. When this arrangement is used, the pump, for convenience of working, should be attached at right angles to the test-tube. It is found that any want of uniformity in the colour of the field produced by the air heated on the sides of the tube can be greatly obviated by lining the inside of the tube with a non-conducting substance, and

keeping it wet. Blotting-paper is found to do very well, as it holds plenty of water for saturating the air, and is a fair non-conductor. When the inside of the tube is lined with it, the field of colour is fairly uniform, owing to the cooling of the sides by the evaporation when the air enters and when expansion is made.

For illumination, no doubt day light is the best when it can be obtained, as gas light is so deficient in blue rays that colour is not well observed. For convenience of observation, it is found to be best to close the far end of the tube with ground glass, and when working with artificial light ground glass must be used.

I have made a few tests with an instrument of this kind, and find it very easily worked; and for many practical purposes it is sufficiently accurate. It cannot, of course, compare with the dust counter for accuracy; but, on the other hand, it is a much less expensive instrument, and tests can be made far more easily with it, and little special knowledge is required. If we wish to get actual figures for the amounts of dust indicated by this instrument, then it must be graduated by a dust counter. The indications at best, however, will only be very rough approximations to the numbers.

There are three ways in which we might graduate this instrument. We might, for instance, make one full stroke of the pump, and note the colour which appeared. This colour would indicate the number of particles. For instance, if there are few particles, one stroke will make the light first blue, then green, then yellow, and then a second blue and green, and finishing with yellow. But if there are a good many particles present, the same amount of expansion will only make the first series of colours to appear, and if a great many particles are present, the one stroke will not give the whole of the first series of colours, but may stop at the blue. If the temperature of the air were always the same, this method might be adopted, but, as we have seen, an allowance would be required to be made for temperature, as with a high temperature the same degree of expansion carries the colour further up the series.

Another method of graduating might be to note the amount of expansion necessary to give any particular colour, say to give the best blue. With few particles a slight expansion gives the blue, while with many particles a much greater expansion is necessary. But here again the effect of temperature comes in. Temperature observations would therefore require to be taken, and a correction made which it might be difficult to carry out in practice.

At present the best plan of graduating seems to be to note the depth of the blue produced, regardless of the amount of expansion required to give it, and use only this quantity as an index. With few particles the colour is pale, and as the particles increase in number the colour increases in depth. Perhaps some addition might be

made to the instrument for estimating more accurately the depth of colour than can be done mentally. This might be done either by means of coloured glasses of different depths for comparison, or in some other way.

A few comparative observations have been made with the koniscope and the dust counter in the impure air of a room. While the number of particles was counted by means of the dust counter, the depth of blue given by the koniscope was noted. A metal tube was fitted up vertically in the room in such a way that it could be raised to any desired height into the impure air near the ceiling so that supplies of air of different degrees of impurity might be obtained. To produce the impurity, the gas was lit and kept burning during the experiments. The air was drawn down through the pipe by means of the air-pump of the koniscope, and it passed through the measuring apparatus of the dust counter on its way to the koniscope. The indications of the two instruments were taken, and are entered in the following table:—

Dust Counter. Particles per c.c.	Koniscope. Depth of colour.
50,000	Colour just visible.
80,000	Very pale blue.
500,000	Pale blue.
1,500,000	Fine blue.
2,500,000	Deep blue.
4,000,000	Very deep blue.

It is probable that the higher numbers are too low, as the measure of the dust counter has a capacity of only 10 c.mm. With so small a measure it is probable that a good many of the particles are lost.

When making a sanitary inspection, the air outside, or wherever the supply was drawn from, would be tested first, and the depth of colour which it gave would be noted. Any increase from that depth would indicate that the air was being polluted, and the amount of increase in the depth of colour would indicate the amount of increase of pollution. Slight colour can be traced though the number of particles be less than 80,000 per c.c., but the colour is not very decided, the condensation producing principally a darkening effect. It should be noted that the above table refers to a koniscope with a test-tube 50 cm. long. An instrument with a tube 1 metre long would be doubly sensitive, and would show colour with fewer particles.

It is thought that the koniscope will be useful for sanitary inspectors, for investigating questions of ventilation in rooms lighted with gas, and for other purposes. As an illustration of what this instrument can tell us, the following experiment may be given. It shows us how we can trace by means of it the pollution taking place in our

rooms by open flames. The room in which the tests were made is 24 × 17 × 13 feet. The object of the tests was to see if the koniscope could tell us anything definite about the degree of pollution at the different parts of the room, and also about the rate at which it was increasing. For this purpose a small tube was arranged so that one end of it could be raised to the ceiling, or into the air of any part of the room from which it was desired to take the air; the other end of the tube was connected with the koniscope.

The first thing to be done was to examine the air of the room before lighting the gas and beginning the tests. On doing this the air at the level of the observer gave a very faint colour, scarcely perceptible; air drawn from within 3 inches of the ceiling gave equally little colour, and the air inside the room gave the same colour as the air outside. The upper end of the tube connected with the koniscope was then raised to within 3 inches of the ceiling, near one end of the room, and the koniscope left attached to the lower end; three jets of gas were now lit in the centre of the room, and observations at once begun with the koniscope. Within thirty-five seconds of striking the match to light the gas the products of combustion had extended to the end of the room; this was indicated by the colour in the koniscope suddenly becoming of a deep blue. In four minutes the deep blue-producing air was got at a distance of 2 feet from the ceiling. In ten minutes there was strong evidence of the pollution all through the room. It was strongly indicated near the windows, owing to the down current of cold air on the glass. This impure down current could be traced to the floor, and onwards to the fireplace; while a pure current could be traced from the door to the fireplace. In thirty minutes the impurity at 9 feet from the floor was very great, the colour being a deep blue.

The wide range of the indications of the koniscope, from pure white to nearly black-blue, makes the estimates of the impurity very easily taken with it; and, as there are few parts to get out of order, it is hoped it may come into general use for sanitary work.

The few experiments I have made with this instrument have clearly pointed out that a window is not an unalloyed blessing as regards the purity of the air in our rooms, however much we may have been in the habit of thinking otherwise. In all cases it has been found that in rooms where gas is burning the air near the window is very impure. This impure down current of air near the window has been traced by the koniscope in all rooms tested. The impurity is caused by the cold air on the window sinking, and drawing down the impure air near the ceiling, and this impure air is mixed with the lower air which we are breathing. This effect is, of course, greatest when the windows are unprotected by blinds, shutters, or curtains. It is evident that though a window may supply pure air when it is open,

it yet does much harm when closed, by bringing down the impure air, which, if undisturbed, would have a less injurious effect.

It is to be regretted that this investigation does not promise to yield much of practical value; nevertheless, as we cultivate not only fruits but flowers, so, for the same reason that we cultivate the latter, it is thought that these experiments will repay the attention of physicists. The colours produced by such simple materials as a little dust and a little vapour are as beautiful as anything seen in nature, and well repay the trouble of reproducing them.

Presents, April 28, 1892.

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May 5, 1892.

The LORD KELVIN, President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair as follows:—

Armstrong, Robert Young, Lieut.-Col. R.E.
Beddard, Frank Evers, M.A.
Fleming, Professor John Ambrose, D.Sc.
Foster, Professor Clement Le Neve, D.Sc.
Gadow, Hans, M.A., Ph.D.
Giffen, Robert, LL.D.
Gotch, Professor Francis, M.A., M.R.C.S.
Herdman, Professor William Abbott, D.Sc.
Hutton, Frederick Wollaston, Capt. R.E.
Joly, John, M.A.
Larmor, Joseph, D.Sc.
Miall, Professor Louis C.
Peach, Benjamin Neve, F.R.S.E.
Pedler, Professor Alexander, F.I.C.
Waller, Augustus D., M.D.

The following Papers were read:—

I. "Transmission of Sunlight through the Earth's Atmosphere. Part II. Scattering at Different Altitudes." By Captain W. DE W. ABNEY, C.B., D.C.L., F.R.S. Received April 7, 1892.

(Abstract.)

In this paper the results of observations made by exposing platinotype paper are recorded, and it is shown that the total intensity of light as thus registered is the same as if observations had been made on a ray of λ 4240 alone. The observations were made at altitudes varying from sea-level to 12,000 feet, in different countries, at different times of the year, and during four to five years. The instrument in which the exposures were made is described, as also the method of deriving the intensity of light from the developed prints. The results of these observations agree closely with those obtained by the measures of the spectrum which was described in Part I of this subject. The value of k in the formula (1) $I' = \epsilon^{-k\lambda^{-4}}$ (from which can be calculated the loss of intensity of a ray of any particular wave-length) was found to be 0·00146 at sea-level. It was also found that k apparently varied as h^2, h being the barometric pressure. A table is attached, showing the value of the transmitted light in the formula (2) $I' = Ia^x$, where a is a constant and x the air thickness in terms of the vertical thickness, μ being the formula $I' = I\epsilon^{-\mu x}$, from which (1) and (2) are both shown to be derived.

Bar. in inches.	μ.	a.	Bar. in inches.	μ.	a.
30	0·154	0·856	24	0·098	0·908
29	0·144	0·866	23	0·090	0·915
28	0·134	0·875	22	0·083	0·922
27	0·124	0·884	21	0·075	0·928
26	0·115	0·891	20	0·068	0·934
25	0·107	0·899	19	0·062	0·940

II. "On the Simultaneity of Magnetic Variations at different places on occasions of Magnetic Disturbance, and on the relation between Magnetic and Earth Current Phenomena." By WILLIAM ELLIS, F.R.A.S., Superintendent of the Magnetical and Meteorological Department, Royal Observatory, Greenwich. Communicated by W. H. M. CHRISTIE, F.R.S., Astronomer Royal. Received April 7, 1892.

(Abstract.)

In this paper the author refers to the ordinary variations of the magnetic elements as observed at Greenwich; the annual progressive change; the diurnal variation—large in summer, small in winter, and also larger when sun spots are numerous and smaller when sun spots are few; the irregular magnetic disturbances and magnetic storms, and the accompanying earth currents; phenomena which are generally similar at other places.

He then invites attention more particularly to magnetic disturbances. Those at Greenwich may, after a calm period, arise gradually, or commence with great suddenness. When sudden, the movement is simultaneous in all elements. The first indication may be a sharp, premonitory, simultaneous movement, followed after a time by general disturbance, or the movement may at once usher in the disturbance. These initial movements are not always great in magnitude, sometimes, indeed, small, but they have a very definite character, and frequently occur nearly instantaneously, as is shown by the character of the photographic traces.

It has been long known that magnetic disturbances occur at the same time over wide areas of the earth's surface, but the accidental comparison in past years of the times of commencement of one or two disturbances at Greenwich with the times at other places has led the author to suppose that the coincidence in time is much closer than had been before supposed, and the definite, and on occasions isolated, character of the initial movement induced him to undertake the collection and comparison of the times of such movements for a number of days at observatories geographically widely separated.

The times of such movements cannot be caught by eye observation without continuous watching of the magnets, so that the photographic registers have to be relied upon, which is better, excepting that the scale of time is necessarily contracted; but, though in individual measures there might be variations, it was conceived that (supposing no systematic error to exist) the mean of a number of comparisons should give a good result. Seventeen days occurring in the years 1882 to 1889 were selected for comparison, the observatories

being those of Toronto, Greenwich, Pawlowsk, Mauritius, Bombay, Batavia, Zi-ka-wei, and Melbourne, and, for a less number of days, Cape Horn (as obtained from the Mission Scientifique du Cap Horn, 1882–83). It was desired to have times for Pola, but it was found that photographic registers during great part of the period did not exist. The variation in time at each place from the mean of times for all places is given for each day. The mean deviation at the different places varies from +2·4 minutes to −2·9 minutes, the agreement between four of the places, Greenwich, Pawlowsk, Mauritius, and Bombay, being very much closer, the mean values of deviation for Greenwich, Pawlowsk, and Bombay differing, indeed, by only 0·1 minute, equivalent to 6 seconds.

The question arises, Are the differences real, or due (considering the contracted time scale) to accidental error? If the magnetic impulse is really simultaneous over the whole earth, it is a striking physical fact, and if not entirely so, the circumstance is no less interesting; but greater attention to accuracy of time scale, or a more extended scale, may be necessary before the point in question can be definitely settled.

A table is added, showing the character of the magnetic movement at the several observatories, from which it appears that at any one place the movements on different days were in most cases similar, though different at different places, indicating on these occasions the occurrence usually of one general type of disturbance.

Reference is made to the question of earth currents. A comparison for thirty-one days, between 1880 and 1891, of cases of sudden magnetic movement and earth current, shows the earth current to precede the magnetic movement by 0·14 minute, equivalent to 8 seconds. The question of the relation between magnetic movements and earth currents is discussed.

The desirability of being able temporarily to obtain, when occasion requires, a more extended time scale for all magnetical and meteorological phenomena is pointed out.

The general result is that in the definite magnetic movements preceding disturbance the magnets at any one place are simultaneously affected; also that in places widely different in geographical position the times are simultaneous, or nearly so, a small constant difference existing at some places which may be real or may be accidental, but the character of which it seems desirable to determine. It is shown also that at Greenwich definite magnetic movements are accompanied by earth current movements which are simultaneous, but that neither magnetic irregularities nor ordinary magnetic variations seem to admit of explanation on the supposition of being produced by the direct action of earth currents

III. "On the Liquation of Metals of the Platinum Group." By EDWARD MATTHEY, F.C.S., F.S.A., Ass. Roy. Sch. Mines. Communicated by Sir G. G. STOKES, Bart., F.R.S. Received March 3, 1892.

(Abstract.)

The author has continued a previous investigation of his own which was published in the 'Proceedings of the Royal Society' ('Roy. Soc. Proc.,' 1890, vol. 47, pp. 180—186).

In the present paper he discusses, in much detail, the effects of the cooling of large masses of the alloys gold-platinum, gold-palladium, platinum-palladium, platinum-rhodium, and gold-aluminium.

The details of manipulation, which were of considerable difficulty, are set forth in detail, as they involved melting masses of metal with high melting points.

The author regrets that time has not enabled him to examine *more* members of each particular series of alloys, so as to present results in fuller detail: in fact, the silver-copper series is the only one upon which anything like exhaustive work has been done.

No doubt, in every series of alloys there is one definite alloy which would yield a uniform mass on cooling, and it is known that in the silver-copper series this alloy (Levol's) contains 718 parts of silver per thousand. It is not certain, however, that this is the eutectic alloy of the series—that is, the one with the lowest melting point—but it is well known that when silver-copper alloys which contain more silver than 718 parts per thousand are cooled, the centre of the solidified mass is richer than the exterior. This is the case with standard silver, for instance, which contains 925 parts of silver per thousand, and it is safe to conclude that an alloy rich in copper is the first to fall out from the mass, and that this alloy sets round the inner surface of the mould, driving a still fluid alloy, rich in silver, to the centre. The general rule in the present results seems to be that, *in the cooling of a fluid mass of two united metals an alloy rich in the more fusible constituents of the mass falls out first, driving the less fusible constituent to the centre.* The gold-platinum alloys (A, B, C, D, and E) seem to be always rich in gold externally.

It is remarkable that the metals of the platinum group do not show much liquation among themselves, but, on the other hand, when gold is united to members of the platinum group, there is evidence of liquation.

The gold-palladium one (F) follows the above rule.

There is evidence that the alloy E, containing 750 parts of platinum and 250 of gold, is near the composition of a true compound, as it shows but little sign of liquation, and is, moreover, hard and brittle,

differing materially from the rest of the series. The purple alloy of gold and aluminium M, $AuAl_2$, is almost certainly a true chemical compound, the solidified mass being as nearly uniform in composition as may be. The uniformity of the alloy (J) of platinum with 10 per cent. of rhodium is of much interest, in view of the important part which the alloy is playing in pyrometric work.

Conducting the experiments, the results of which are embodied in the present paper, has been very laborious, and although, as already stated, no complete series of the alloys of any two metals has been examined, quite sufficient data have been collected to afford valuable guidance to the metallurgist, who will now know what behaviour may be expected from the other members of the groups of the alloys in question. The gold-platinum series of alloys are of industrial importance, as native gold is so often associated with platinum, and it is somewhat surprising to find that assays made on pieces of metal cut from the exterior of an ingot cannot be trusted to represent the composition of the mass. The aim of the investigation has been to show, that notwithstanding the great difficulty which attends the preparation of alloys of metals with very high melting points, it is possible to elicit from them the same kind of information which has proved to be so useful in the case of the more ordinary and tractable alloys.

IV. "The Potential of an Anchor Ring." By F. W. Dyson, Fellow of Trinity College, Cambridge. Communicated by Professor J. J. Thomson, F.R.S. Received March 19, 1892.

(Abstract.)

If r, θ, ϕ be the coordinates of any point outside an anchor ring, whose central circle is of radius c, then

$$\int_0^\pi \frac{d\phi}{\sqrt{(r^2+c^2-2cr\sin\theta\cos\phi)}}$$

and

$$\cos\phi\int_0^\pi \frac{\cos\phi\, d\phi}{\sqrt{(r^2+c^2-2cr\sin\theta\cos\phi)}}$$

are solutions of Laplace's equation, which are finite at all external points and vanish at infinity.

Let these be called I and J.

Then $d\mathrm{I}/dz$ and $d\mathrm{J}/dz$ are also solutions of Laplace's equation.

Four sets of solutions are obtained by differentiating these integrals any number of times with respect to c.

Thus, I, $d\mathrm{I}/dc$, $d^2\mathrm{I}/dc^2$, &c., are all solutions of Laplace's equation, finite at all external points and vanishing at infinity.

This fact is applied to find the potential of an anchor ring in external space, the velocity potential of a ring moving in any manner in an infinite fluid, the potential of a charged conductor in the form of an anchor ring, and the annular form of rotating fluid.

Fig. 1.

2. Let a section be taken through the axis of the ring, cutting the central circle at C. Let P be a point in this section, and let

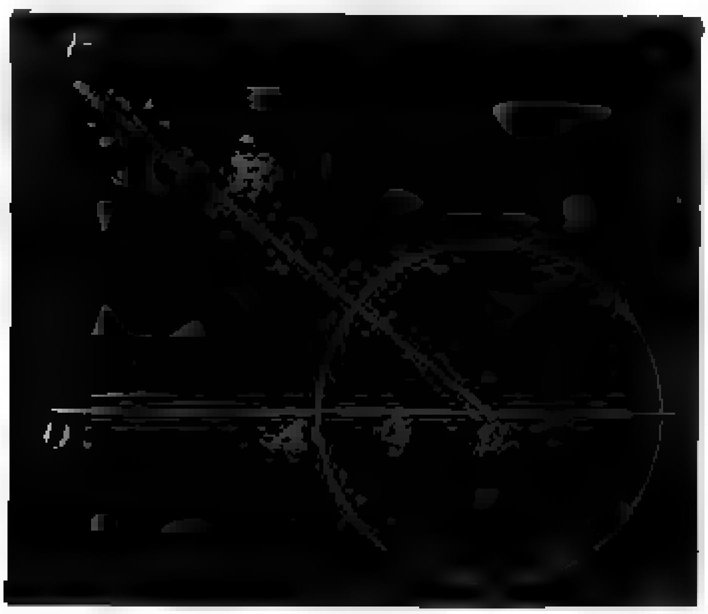
$$\mathrm{CP} = \mathrm{R}, \qquad \mathrm{OC} = c, \qquad \text{and } \mathrm{OCP} = \chi.$$

When $\mathrm{R} < c$, the integrals

$$\int_0^{\pi} \frac{d\phi}{\sqrt{(r^2+c^2-2cr\sin\theta\cos\phi)}} \quad \text{and} \quad \int_0^{\pi} \frac{\cos\phi\, d\phi}{\sqrt{(r^2+c^2-2cr\sin\theta\cos\phi)}}$$

are expanded in ascending powers of R/c; the expansions of the integrals $d\mathrm{I}/dc$, $d^2\mathrm{I}/dc^2$, &c., are deduced from these.

Fig. 2.



In this way the values of the integrals at the surface of the ring are found and the required boundary conditions satisfied.

3. The potential of an anchor ring at any point on its axis is shown to be

$$\frac{2M}{\pi}\int_0^\pi \frac{\sin^2\psi\, d\psi}{\sqrt{(R^2-2aR\cos\psi+a^2)}},$$

where $R = CP$ and $a = CA$.

This is reduced to elliptic functions and expanded in ascending powers of a/R, giving

$$V = M\left\{\frac{1}{R}-\frac{1}{8}\frac{a^2}{R^3}-\frac{1}{64}\frac{a^4}{R^5}-\text{\&c.}\right.$$
$$\left.-2\,\frac{1^2.3^2\ldots\ldots(2n-3)^2(2n-1)}{2^2.4^2\ldots\ldots(2n-2)^2(2n)^2(2n+2)}\frac{a^{2n}}{R^{2n+1}}-\text{\&c.}\right\}$$

from which it is deduced that at any external point r,θ

$$V = \frac{M}{\pi}\left\{\int_0^\pi \frac{d\phi}{\sqrt{(r^2+c^2-2cr\sin\theta\cos\phi)}}\right.$$
$$+\frac{a^2}{8}\frac{d}{c\,dc}\int_0^\pi \frac{d\phi}{\sqrt{(r^2+c^2-2cr\sin\theta\cos\phi)}}-\text{\&c.}$$
$$\left.+(-1)^{n+1}\frac{2a^{2n}}{2n+2}\frac{1.3.5\ldots\ldots(2n-3)}{2^2.4^2.6^2\ldots\ldots(2n)^2}\left(\frac{d}{c\,dc}\right)^n\int_0^\pi \frac{d\phi}{\sqrt{(r^2+c^2-2cr\sin\theta\cos\phi)}}+\text{\&c.}\right\}$$

This series of integrals is very convergent; the first three are reduced to elliptic functions, and the equipotential surfaces are drawn for the ratios $\frac{1}{5}$, $\frac{2}{5}$, $\frac{3}{5}$, $\frac{4}{5}$, 1, of a/c.

4. The potential of a conductor in the form of an anchor ring is shown to be of the form

$$A_0 I + A_1 a^2\frac{dI}{c\,dc}+A_2 a^4\left(\frac{d}{c\,dc}\right)^2 I+\text{\&c.}$$

A_0, A_1, A_2, &c., are determined, and it is shown that on the surface of the ring

$$V = \frac{A_0}{c}\left\{\lambda_0+2-\frac{4\lambda_0+8\lambda_0+5}{2^4}\sigma^2+\frac{192\lambda_0+416\lambda_0^2+448\lambda_0+179}{2^{11}}\sigma^4+\ldots\right\}$$

and

$$\frac{dV}{dn} = \frac{A_0}{ac}\left\{-1+\left(\frac{2\lambda_0+1}{2}-\frac{24\lambda_0^2+7}{2^6}\sigma^2\right)\left(\frac{\sigma^2}{2}+\sigma\cos\chi\right)\right.$$
$$+\left(\frac{8\lambda_0+1}{2^4}-\frac{36\lambda_0^2+\lambda_0+13}{2^7}\sigma^2\right)\sigma^2\cos 2\chi$$
$$\left.+\frac{16\lambda_0-3}{2^6}\sigma^3\cos 3\chi+\frac{11(8\lambda_0-1)}{2^{10}}\sigma^4\cos 4\chi+\text{\&c.}\right\}.$$

and the

$$\text{charge} = \pi A_0, \text{ where } \sigma \text{ denotes } a/c \text{ and } \lambda_0 = \log \frac{8c}{a} - 2.$$

5. The velocity potential of a ring moving in an infinite fluid is next found—

(1) When the ring moves parallel to its axis;
(2) When the ring moves perpendicularly to its axis;
(3) When the ring rotates about a diameter of its central circle.

The stream line function is found—

(1) For motion parallel to the axis;
(2) For the cyclic motion through the ring.

Thus, in the first case it is shown that

$$\Phi = wa^2 \left\{ \left[1 + \frac{4\lambda_0+1}{8}\delta_0^2 + \frac{16\lambda_0^2+104\lambda_0+33}{64}\delta_0^4\right]\frac{dI_1}{dz} - \frac{12\lambda_0+9}{16}\delta_0^4\frac{dI_2}{dz} \right.$$
$$\left. + \left[\frac{\delta_0^4}{16} + \frac{112\lambda_0+91\frac{1}{2}}{256}\delta_0^6\right]\frac{dI_3}{dz} - \frac{\delta_0^6}{48}\frac{dI_4}{dz} + \frac{5\delta_0^8}{1024}\frac{dI_5}{dz}\right\},$$

where $$I_1 = \int_0^\pi \frac{c\,d\phi}{\sqrt{(r^2+c^2)-2cr\sin\theta\cos\phi}},$$

and $$I_n = \left(2c\frac{d}{dc}\right)^{n-1} I_1,$$

and $$\delta_0 = a/2c.$$

The kinetic energy is calculated in each case as far as δ_0^4, the result being that

$$2T = P(u^2+v^2) + Rw^2 + A(\omega_1^2+\omega_2^2) + K\kappa^2,$$

where

$$P = \frac{M}{2}\left\{1 - \frac{12\lambda_0+1}{4}\delta_0^2 - \frac{72\lambda_0^2-24\lambda_0+7}{16}\delta_0^4 - \dots\right\},$$

$$R = M\left\{1 - \frac{4\lambda_0+1}{4}\delta_0^2 + \frac{16\lambda_0^2+8\lambda_0-23}{32}\delta_0^4 + \dots\right\},$$

$$A = M\frac{c^2}{2}\left\{1 - \frac{12\lambda_0+7}{4}\delta_0^2 + \frac{144\lambda_0^2-36\lambda_0+17}{32}\delta_0^4 + \dots\right\},$$

$$K = \rho\frac{c}{2}\left\{\lambda_0 - \frac{4\lambda_0^2+8\lambda_0+1}{4}\delta_0^2 - \frac{64\lambda_0^3+188\lambda_0^2+192\lambda_0-49}{128}\delta_0^4 + \dots\right\},$$

where M is the mass of the fluid displaced.

The linear momentum of the cyclic motion is also found to be

$$\pi c^2 \kappa \rho \left\{ 1 - 4(\lambda_0+1)\,\delta_0^2 - \frac{4\lambda_0^2+5\lambda_0-2}{2}\,\delta_0^4, \text{ \&c.} \right\}.$$

A few cases of motion of the ring are then discussed.

6. The annular form of fluid rotating in relative equilibrium is next considered, when the radius of the mean circle of the ring is at least twice as great as the mean radius of the cross-section.

The equation of the cross-section is assumed to be

$$\rho = a\,(1+\beta_1 \cos\chi + \beta_2 \cos 2\chi + \ldots .),$$

where β_1, β_2, &c., are small quantities.

Taking the centre of gravity of the cross-section as origin, β_1 is seen to be of the 5th order, and it is shown that, as far as the 4th order,

$$\frac{\omega^2}{\pi} = \left(\lambda_0+\frac{3}{4}\right)\sigma^2 - \frac{1}{8}\left(\lambda_0+\frac{19}{12}\right)\sigma^4,$$

$$\beta_2 = \frac{\frac{5}{8}\sigma^2\left\{\left(\lambda_0+\frac{7}{12}\right)+\frac{5\sigma^2}{48}\left(\lambda_0-\frac{107}{120}\right)\right\}}{1-\left(\lambda_0+\frac{1}{2}\right)\sigma^2},$$

$$\beta_3 = \frac{5}{128}\left(\lambda_0-\frac{7}{24}\right)\sigma^3,$$

$$\beta_4 = \frac{75\lambda_0^2+90\lambda_0+23}{256}\,\sigma^4.$$

The shape of the cross-section is roughly elliptical, the major axis of the ellipse being perpendicular to the axis of revolution.

V. "On the Residues of Powers of Numbers for any Composite Modulus, Real or Complex." By GEOFFREY T. BENNETT, B.A. Communicated by Professor CAYLEY, F.R.S. Received April 8, 1892.

(Abstract.)

The present work consists of two parts, with an Appendix to the second. Part I deals with real numbers, Part II with complex.

In the simple cases, when the modulus is a real number which is an odd prime, a power of an odd prime, or double the power of an odd prime, we know that there exist primitive roots of the

modulus: that is, that there are numbers whose successive powers have for their rests the complete set of numbers less than, and prime to, the modulus. A primitive root may be said to generate by its successive powers the complete set of rests. It is also known that in general, when the modulus is any composite number, though primitive roots do not exist, there may be laid down a set of numbers, which will here be called *generators*, the products of powers of which give the complete set of rests prime to the modulus.

The principal object of Part I is to investigate the relations which must subsist among any such set of generators; to determine the most general form that they can take; to show how to form any such set of generators, and, conversely, to furnish tests for the efficiency, as generators, of any given set of numbers. Other results which are obtained as instrumental in effecting these objects, such as the determination of the number of numbers that belong to any exponent, may also possess independent interest.

The object of Part II is to make, for complex numbers, an investigation which shall be as nearly as possible parallel to that of Part I for real numbers. Much of the work of Part I may be applied immediately to complex numbers; of the rest some will need slight modification, and some will need replacing by propositions leading to corresponding results. Of those cases which thus call for independent treatment, the most noticeable is that of the modulus $(1+i)^\lambda$, which is the complex analogue of the real modulus 2^λ.

The work is put in the form of a series of propositions, and is started almost from first principles. The early part is consequently elementary, but the advantages of completeness and ease of reference may be more than sufficient to compensate for this. A large number of illustrative examples are given. These will sometimes, perhaps, assist in elucidating the symbolical proofs which they follow: in all cases they will help to maintain clearly the actual arithmetical meaning of the results arrived at, a meaning which may easily seem obscure if it be noticed only in its symbolical and generalised form.

The Appendix contains tables of indices for complex numbers for all moduli whose norms do not exceed 100.

Presents, May 5, 1892.

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May 12, 1892.

Mr. JOHN EVANS, D.C.L., LL.D., Treasurer and Vice-President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Transformers." By JOHN PERRY, F.R.S., Professor of Mechanical Engineering and Applied Mathematics, City and Guilds Technical College, Finsbury. Received March 23, 1892.

I. In a transformer with many circuits, of resistances in ohms and numbers of turns R_1, N_1, R_2, N_2, &c.; let the currents at any instant be C_1, C_2, &c., ampères, and let the independent electromotive forces e_1, e_2, &c., volts be maintained in them; let I be the total magnetic induction which threads through all the circuits (10^8 C.G.S. units being taken as the unit of induction). The equations to the circuits are :—

$$e_1 = R_1C_1 + N_1\theta I, \quad e_2 = R_2C_2 + N_2\theta I, \quad \&c.,$$

using θ for d/dt.

Multiplying each equation by its N and dividing by its R and adding, we find, using A for the ampère turns $N_1C_1 + N_2C_2 + \&c.$ and q for $\frac{N_1^2}{R_2} + \frac{N_2^2}{R_2} + \&c.$,

$$A + q\theta I = \Sigma(N_i/R_i)e_i \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (1).$$

If the law connecting A and I is known, it is possible to calculate I from (1), and then substituting for I in each of the circuit equations, each of the currents is known.

Instead of e_i being the electromotive force in a complete circuit, it may be the potential difference maintained at the terminals of a circuit and the letter V_i will then be used, the resistance between the terminals being R_i. If e_i or V_i has any value, the circuit is called a primary circuit. When no independent electromotive force is maintained in a circuit, it is called a secondary circuit.

II. Substitution of one coil for two or more:—It is evident from the way in which (1) is derived, that instead of a primary coil (V_1, N_1, R_1) and a secondary (N_2, R_2), if we substitute a primary (V_1, n_1, r_1) and if $n_1/r_1 = N_1/R_1$ and also if $n_1^2/r_1 = N_1^2/R_1 + N_2^2/R_2$, then the currents in all the other coils will be the same as before. We can therefore replace a primary and any group of secondaries by one primary coil.

It is also evident that if any group of secondaries (N_p, R_p), (N_t, R_t), &c., be replaced by one secondary (n_2, r_2) such that $n_2^2/r_2 = N_p^2/R_p + N_t^2/R_t +$ &c., all the other currents will remain as before. It is, in fact, evident that if for groups of primaries and secondaries we substitute one primary coil of suitable potential difference at its terminals and resistance and number of turns, all the currents in the remaining coils will remain the same.

If two secondaries of numbers of turns N_p and N_t and internal resistances r_p and r_t have the same terminals, and together supply a current to an outside circuit, for them we may substitute one secondary of n turns and internal resistance r ohms with a permanent short circuit of resistance ρ between its terminals, if

$$n = (N_p^2/r_t + N_t^2/r_p)/(N_p r_t + N_t r_p),$$

$$r = r_p r_t (N_p^2 r_t + N_t^2 r_p)/(N_p r_t + N_t r_p)^2,$$

and

$$\rho = (N_p^2 r_t + N_t^2 r_p)/(N_p - N_t)^2.$$

Hence no good results can be expected from compound winding. If, instead of requiring a perfect similarity in all respects, we desire a substitution which is almost absolutely perfect in practical cases, we can obtain it by the use of a single coil with no short circuit outside, and therefore without the excessive waste of power which occurs when two coils of different numbers of turns are compounded. This last result is obtained by regarding A as a negligible term in (1).

Two coils in parallel to one another, used as a primary or secondary, will evidently waste energy in idle currents inside the transformer unless they are exactly equal in their numbers of turns.

III. If condensers are connected with any of the circuits the

most general case is this:—Take the internal resistance of the sth coil to be r_s; let it have a condenser outside of capacity K_s in series with a resistance ρ_s'' and let there be a resistance ρ_s' in parallel with ρ_s'' and the condenser. Then, in the general expression, instead of R_s we must use

$$r_s + (\rho_s'\rho_s''K_s\theta + \rho_s')/\{(\rho_s' + \rho_s'')K_s\theta + 1\}.$$

Having found C_s, we must remember that the part passing through ρ_s' is $(\rho_s''K_s\theta + 1)C_s/\{(\rho_s' + \rho_s'')K\theta + 1\}$. Everywhere, when any resistance r is spoken of, if there be also a coefficient of self-induction l (or in case it is an internal resistance r_s that is spoken of, should there be magnetic leakage), we must use instead of r the expression $r + l\theta$. Our formulæ, in case there is magnetic leakage, in case there is self-induction, or in case there are condensers, really require that a complicated operation

$$\frac{a + b\theta + c\theta^2 + d\theta^3 + e\theta^4 + f\theta^5 + \&c.}{a' + b'\theta + c'\theta^2 + d'\theta^3 + e'\theta^4 + f'\theta^5 + \&c.}$$

should be performed upon a function of the time. Now the functions with which engineers deal are periodic functions, and the evanescent parts of the answers may be left out of consideration. As any periodic function of the time is the sum of simple harmonic functions, we have only to perform the above operations upon functions like $\sin(kt+m)$. But it is obvious that the complicated operation when performed on $\sin(kt+m)$ reduces to

$$\frac{(a - ck^2 + ek^4 - \&c.) + (b - dk^2 + fk^4 - \&c)\,\theta}{(a' - c'k^2 + e'k^4 - \&c.) + (b' - d'k^2 + f'k^4 - \&c.)\,\theta},$$

an operation which is very easy to perform.

Writing it in the form $(p + q\theta)/(\alpha + \beta\theta)$, the result of operating upon $\sin(kt+m)$ is to convert it into

$$\sqrt{\left(\frac{p^2 + q^2k^2}{\alpha^2 + \beta^2k^2}\right)}\sin\left(kt + m + \tan^{-1}\frac{qk}{p} - \tan^{-1}\frac{\beta k}{\alpha}\right)$$

Hence the most complicated cases are readily worked out when numerical examples are taken. Engineers are in the habit of speaking of the epoch m as a lead when it is positive, and as a lag when it is negative. The reciprocal of the periodic time is called the *frequency*, and k is $2\pi \times$ frequency.

IV. Law connecting A and I:—

1. If μ, the magnetic permeability of the iron, is constant, let σ stand for $4\pi\alpha\mu 10^{-9}/\lambda$ where α is the area of cross-section of the iron in square centimetres, and λ is the average length of the induction

solenoids in centimetres. The curve which on squared paper illustrates the magnetic law is a straight line, and $I = \sigma A$.

2. If the curve is not a straight line, that is, if there is some indication of saturation, and there generally is some saturation where β the induction per square centimetre exceeds 2×10^{-6} (or 2000 C.G.S. units), it will be found that a good approximation to actual facts is obtained by assuming that when $I = I_0 \sin x$, $A = A_0(\sin x - b \sin 3x + m \sin 5x + \&c.)$ where $I_0/A_0 = \sigma$, a constant, and n is any quantity which increases continuously. Thus it will be found that taking only one harmonic, $b = 0{\cdot}2$ gives a close approximation to what may be the actual case in transformer problems. Taking two harmonics, $b = 0{\cdot}2$ and $m = 0{\cdot}05$, gives a better result. There is no hysteresis in such cases.

3. If the curve is a simple hysteresis loop such as may be obtained in slowly-performed cyclic magnetisation, on the assumption that $I = I_0 \sin x$, one of my students, Mr. Fowler, has found A in terms of $\sin(x+e_1)$, $\sin(2x+e_2)$, &c., a rather complicated expression which I need not give. But another student, Mr. Field, finds that the approximation $A = A_0\{\sin(x+f) - b \sin 3x + m \sin 5x\}$ is sufficiently accurate for many purposes of calculation if $b = 0{\cdot}2$ and $m = 0{\cdot}05$. However complicated the law may be, it can be expressed in the shape :—if $I = \Sigma I_i \sin(ix+e_i)$ then to the ith term in I there correspond the terms in A, $(I_i/\sigma_i)\{\sin(ix+e_i+f_i) - b_i \sin 3ix + m_i \sin 5ix + \&c.\}$.

Our first assumption of constant permeability applied to a transformer with one primary and many secondaries causes equation (1) to become $A = (1+q\sigma\theta)^{-1}N_1V/R_1$, and hence

$$-C_s = (N_sN_1/R_sR_1)\{\sigma\theta V/(1+q\sigma\theta)\}.$$

I will, for sake of illustration, consider a 1500-watt transformer, which is a specimen of many in use. One primary coil $R_1 = 27$ ohms, $N_1 = 460$ turns. Internal resistance of the one secondary coil $= 0{\cdot}067$ ohm, $N_2 = 24$ turns. Effective primary potential difference $= 2000$ volts or $V = 2828 \sin kt$. Frequency about 95 per second, or $k = 600$, $\alpha = 360$ square centimetres, $\lambda = 31$ centimetres. It will be found that the highest value of β, the induction per square centimetre, is $2{\cdot}755 \times 10^{-5}$ (or 2755 C.G.S. units). If there are neither condensers, nor self-inductions, nor magnetic leakage, and if $\mu =$ 2000 (it will not much affect our results to take $\mu = 1500$ or 3000), then $\sigma = 3 \times 10^{-4}$; if there is no load on the transformer $q = 7837$, and if there is full load, when $R_2 = 6{\cdot}8$ ohms, $q = 7922$. It is evident from these numbers that in practical cases $1+q\sigma\theta$ is nearly the same as $q\sigma\theta$. Neglecting the 1 means neglecting an alteration of 1/500,000th of the amplitude and a lag of 1/200th of a degree. We see that in this case neglecting the term A in equation (1) is quite

allowable, and if such a transformer had one primary and many secondary circuits, we might at once write

$$I = N_1 V_1 / R_1 q\theta \qquad \dots\dots\dots\dots\dots\dots \quad (2),$$

$$-C_s = N_s N_1 V_1 / R_s R_1 q \qquad \dots\dots\dots\dots\dots\dots \quad (3).$$

Of course, with any law of magnetisation these are the answers if A may be neglected in equation (1).

Taking the third assumption, the simplest hysteresis cycle, which when $f = 0$ is also the second assumption. If $I = A_0 \sigma \sin kt$, (1) gives for V_1 the value

$$V_1 = \frac{A_0 R_1}{N_1} \{\cos f \sin kt + (\sin f + q\sigma k) \cos kt - b \sin 3kt + m \sin 5kt\}.$$

If $k = 600$ or a frequency of 95 per second, the lowest value of $q\sigma k$ for the above-mentioned transformer is 1411, sothat taking f of any value whatever, and b and m even much larger than the values given above, it is obvious that for all practical purposes, certainly in the case where it is the effective value of V_1, which is important, the above expression for V_1 is the same as if f, b, and m were zero. No doubt V_1 does possess small traces of the higher harmonics, but taking the above values for b and m, and taking $f = 20°$, I find that the error in neglecting f, b, and m, in calculating the effective voltage, is utterly insignificant. I have applied to this problem the most complex law of magnetisation which I could formulate which was at all likely to be true, and in all cases I have arrived at the same result:—Given the voltage V_1 at the terminals of a primary coil of a transformer with many secondary coils, the induction and the secondary currents may be calculated from 2 and 3, which were worked out on the assumption of constant permeability, and in all cases the error in the effective values is exceedingly small.

V. Magnetic Leakage.—If all the induction due to the current in any coil does not thread through all the other coils, the effect of leakage is obtained by assuming that each N is really less than the number of turns, and that there is some self-induction in each circuit in addition to $N^2\sigma$. If the additional inductances are l_1, l_2, &c., then, in our expression R_1, R_2, &c., will be replaced by $R_1 + l_1\theta$, $R_2 + l_2\theta$, &c. Thus, in the above-mentioned 1500-watt transformer, let I_0 represent the induction when there is no load on the transformer. Assume that 1 per cent. of the induction due to the primary current escapes the secondary, and that 1 per cent. of the induction due to the secondary current escapes the primary, or that $l_1 = 10^{-2} N_1^2 \sigma = 0{\cdot}6348$, $l_2 = 10^{-2} N_2^2 \sigma = 1{\cdot}738 \times 10^{-3}$. Let I be the induction at full load, or when $R_2 = 6{\cdot}8$ ohms. When there is no magnetic leakago $I = 0{\cdot}99 I_0$; when there is 1 per cent. magnetic leakage $I/I_0 =$

$(1+2{\cdot}54\times10^{-4}\theta)/(1{\cdot}0108+5{\cdot}08\times10^{-4}\theta)$. Assuming I_0 to be $\sin kt$, I give the values of I for various values of k.

k.	I.
100	$0{\cdot}988 \sin (kt - 1^\circ{\cdot}4)$
300	$0{\cdot}981 \sin (kt - 4^\circ{\cdot}2)$
600	$0{\cdot}958 \sin (kt - 8^\circ{\cdot}1)$
1000	$0{\cdot}912 \sin (kt - 12^\circ{\cdot}4)$
2000	$0{\cdot}782 \sin (kt - 18^\circ{\cdot}2)$
4000	$0{\cdot}628 \sin (kt - 18^\circ{\cdot}1)$

VI. Eddy Currents.—There is eddy current loss of power in all the conducting masses near a transformer. If we assume that the induction per square centimetre β is the same everywhere, and if it follows the law β (in C.G.S. units) $= \Sigma a_i \sin (ikt+e_i)$, the average power in watts wasted in eddy currents in the iron per cubic centimetre is $6{\cdot}25\times10^{-13}r^2k^2\Sigma i^2a^2_i$, if the specific resistance of the iron is taken to be 10^4. The iron is supposed to be of wire of radius r centimetres.

The average power wasted in the iron is less at high temperatures, being inversely proportional to the specific resistance of the iron. It is evidently proportional to the square of the effective primary voltage in the unloaded transformer, but, of course, magnetic leakage causes a diminution when the transformer is loaded. In fact, the eddy current loss is always proportional to the square of the effective electromotive force in the secondary circuit. In applying the rule, it is to be remembered that the induction is not uniform in the section of a wire, nor is the average induction in each wire the same for all the wires, and therefore the real loss of power in the iron by eddy currents is always greater than the result of applying the above formula.* If we assume that one secondary circuit closed upon itself represents all the eddy current circuits, assuming the truth of the above formula is the same as assuming no magnetic leakage between the primary and this eddy current circuit. Assuming magnetic leakage gives the more correct result, which is practically that the magnetisation is greater in wires near the surface of the mass of iron than it is internally, and also that the induction is greater near the skin of each wire.

* [*Added April* 29, 1892.—I have often urged the importance of the consideration of the want of uniformity of induction in the iron; but even in February last, when I brought it before a meeting of the Institution of Electrical Engineers, it was looked upon as unimportant, and I had made no calculations which would enable me to prove its importance. The calculations of Prof. J. J. Thomson concerning the eddy currents in thin plates, published in 'The Electrician' of April 8, 1892, place the matter beyond doubt.]

Such experiments as have hitherto been made do not enable us to say to what extent the eddy current loss is increased by this cause. In the above-mentioned 1500-watt transformer there is 10 per cent. less eddy current waste at full load than at no load, because of the change in q^2 and also because of magnetic leakage, and there may be 50 per cent. less eddy current waste due to the higher temperature of the iron.

VII. Unloaded Transformer.—When there is a fair load on the transformer, the primary current may be calculated on the assumption that the permeability is constant, without much error. But when there is no load, or small load, on the transformer it is necessary to know the law connecting A and I. Everybody who has studied this subject takes A in an unloaded transformer as being N_1C_1. Now, in reality, this is an assumption that there are absolutely no eddy currents; but as there are always eddy currents, however finely the iron may be divided, and even small eddy currents produce great effects upon C_1, the magnetic law which has been deduced from experiments on this assumption must be quite untrue. Taking eddy currents effect as represented by a circuit (n, r) with no magnetic leakage; taking as our magnetic law that, when $I = g\sigma \sin x$,

$$A = g(\sin x - b \sin 3x + m \sin 5x),$$

if V is a simple sine function of the time, I is so also with very great accuracy, but not perfect accuracy. Assuming that I is a simple sine function, the neglected terms in V may be calculated; this serves no useful purpose, so far as I can see at present, as they are so insignificant. The only problem of importance is really the calculation of C_1, assuming that with great, but not perfect, accuracy V has the value $V_0 \sin kt$. Our equations are $V = R_1C_1 + N_1\theta I$ and $0 = rc + n\theta I$, and hence $N_1V_1/R_1 = A + q\theta I$, where $A = N_1C_1 + nc$ and $q = N_1^2/R_1 + n^2/r$. Now n^2/r is negligible in comparison with N_1^2/R_1 in transformers, and we may take $q = N_1^2/R_1$. Hence $-I = (V_0/N_1k) \cos kt$ very nearly, and if $e = n^2\sigma k/r$, being called the eddy current effect, f being the hysteresis term,

$$C_1 = (V_0/N_1^2\sigma k)\{\sqrt{(1+2e\sin f+e^2)} \sin [kt - 90 + \tan^{-1} (\tan f + \frac{e}{\cos f})] - b \cos 3kt - m \cos 5kt\} \quad \ldots\ldots\ldots \quad (4).$$

We see that the effect of eddy currents without hysteresis is to increase the amplitude of the fundamental term in C_1, and to produce a lead of $90° - \cot^{-1} e$, whereas the effect of hysteresis without eddy currents is to keep the amplitude unaltered, and to produce a lead f. If f is put equal to 0, that is if we assume no hysteresis, we obtain

results which seem to be in accordance with such experimental observations as have yet been made.

The effective current C' (if V' is the effective voltage) with constant permeability is $V'/N^2\sigma k$; with hysteresis (or with no hysteresis but some saturation of the iron) but no eddy currents, $C' = 1{\cdot}02V'/N^2\sigma k$, taking b as $0{\cdot}2$; with eddy currents and hysteresis, $C' = V'\sqrt{(1{\cdot}04+2e\sin f+e^2)}/N^2\sigma k$.

The average power given to the choking coil or average value of VC is $V'C'\dfrac{e+\sin f}{1+e^2+2e\sin f}$, neglecting the small terms due to b and m.

Probably, in transformers there are always traces of the term in $3kt$ and the higher harmonics in both V and I, but they certainly must exist in either V or I, whether the transformer is loaded or unloaded. In the loaded transformer, magnetic leakage causes considerable diminution in the higher harmonics of I, and this may increase them in V.

It seems that in a choking coil with a finely-divided iron core, we have found what has been long looked for, a method of increasing frequency by mere magnetic means. A condenser shunting a non-inductive part of the circuit would receive currents in which the higher harmonics would be greatly magnified in importance. To show the magnitude of the terms in (4) I will take the above-mentioned 1500-watt transformer, in which $q_0 = 7783$. Taking $f = 0$ or no hysteresis, the power wasted in eddy currents being $n^2V_0^2/2rN_1^2$, let this be 40 watts; then $n^2/r = 2{\cdot}1168$ when $V = 2828$. The eddy current coil therefore which would replace all the eddy current circuits is a coil of two turns whose resistance is about $1{\cdot}9$ ohms, short circuited on itself.

$e = 0{\cdot}38$ if $k = 600$. Assuming constant permeability and no eddy currents and no hysteresis $C_1 = 0{\cdot}07398\sin(kt-90^\circ)$, with some saturation and eddy currents but no hysteresis

$$C_1 = 0{\cdot}07911\sin(kt-69^\circ{\cdot}2)-0{\cdot}014796\cos 3kt-0{\cdot}003695\cos 5kt.$$

I have taken $b = 0{\cdot}2$ and $m = 0{\cdot}05$.

The primary potential difference V is never a simple sine function of the time. Besides the important term in $\sin kt$ there are small terms of higher frequency, and at least one term of lower frequency equal to the number of turns of the armature per second. The tendency of the forces acting on coils in series on an armature is to produce greater dissonance at greater loads, but it may be assumed that in good machines the fastenings are sufficiently rigid, and the coils and pole pieces so nearly alike, that there is always very little dissonance.

The primary potential difference, instead of being $2828\sin 600t$ in the above case, may be

$$V = 100\sin 20t+2823\sin 600t+200\sin 1800t,$$

the effective potential difference, as measured by a voltmeter, being the same in the two cases. The induction, if there is no magnetic leakage, will be

$$-\mathrm{I} = (\mathrm{N}_1/\mathrm{R}_1 q)(5\cos 20t + 4\cdot 7\cos 600t + 0\cdot 11\cos 1800t),$$

the term which was so insignificant, which had only 1/800th of the importance of the most important term in practically estimating the volts, is now greater than what is usually taken to be the greatest term when we come to deal with the actual induction. Magnetic leakage will not much affect this condition of things, but it will greatly diminish the importance of the higher harmonics.

When experimenters say that they keep the primary volts constant, they mean that they keep the effective primary volts constant. It is obvious from the above considerations that different methods of keeping effective volts constant will produce very different kinds of induction. The effects produced by an exciting current in a choking coil or unloaded transformer are evidently very complicated. Let the dynamo have a perfectly pure simple harmonic law of electromotive force; we have seen that even when no hysteresis is assumed, the current will possess large harmonics and the induction possesses corresponding harmonics. The energy wasted in the creation of these harmonics may be called "hysteresis" loss, but it cannot be altogether the same as the hysteresis loss in slowly-performed cycles of magnetisation; it will be different if the dynamo does not follow a simple harmonic law in its electromotive force, and the apportioning of the small higher harmonics to the primary voltage and to the induction must greatly depend upon the self-induction of the dynamo machine.

II. "On the probable Effect of the Limitation of the Number of Ordinary Fellows elected into the Royal Society to Fifteen in each Year on the eventual total Number of Fellows." By Lieut.-General R. STRACHEY, R.E., F.R.S. Received April 13, 1892.

The discussions that arose in connection with the revision of the Statutes of the Royal Society during the years 1890 and 1891, led me to endeavour to obtain definite data on which to found a trustworthy opinion as to the effect of the existing limitation of the number of yearly admissions, on the eventual total strength of the Society, and the probable result of increasing the number beyond fifteen, the present limit.

The facts bearing on this subject, so far as I have been able to

collect them from the records of the Society, are embodied in the Tables annexed to this communication, for the proper appreciation of the significance of the figures in which a few preliminary explanations are necessary.

The Anniversary of the Society being fixed for the 30th November in each year, the customary record of the number of Fellows for any year refers to the number on that date. I have throughout regarded the date to which this number applies as being the 1st January of the following year.

The annual election of Ordinary Fellows usually takes place in the first or second week of June in each year. I have considered the date to be the 1st January of the same year.

The lapses, whether from death or other causes, have been treated as having occurred at the end of the calendar year in which they take place.

These assumptions have been made to simplify the various computations that the investigation required (which have been sufficiently troublesome as it is), and owing to the considerable period dealt with, forty-three years, the results will not, I believe, be sensibly affected thereby.

Unless it is otherwise specifically stated, the numbers refer exclusively to the *Ordinary Fellows*, elected at the regular Annual Meetings fixed for the purpose.

So far as I have been able to ascertain (for the earlier records in many particulars are defective), the number of Ordinary Fellows elected since 1848 has been 15 in each year, except on four occasions; in two years the number having been 14, and in two years 16; the average, therefore, is 15 yearly.

During the period since 1848, the number of *Royal* and *Honorary* Fellows has been about 5, and the *Foreign* Members about 50; these are included in the total number of Fellows shown in the Annual Reports of the Council, but will not be further considered in what follows.

The rules under which certain privileged classes have been admitted as Fellows, in addition to the *Ordinary* Fellows, have varied somewhat since 1848, but at present, apart from the persons eligible for the classes of Fellows above excluded, the only persons so privileged are Privy Councillors. The total number of *Privileged* Fellows elected since 1848 seems to have been 75, which for 43 years gives an average of 1·75 per annum.

Table I contains a summary of the available data relating to the total number of Fellows since 1848.

The total number, excluding Royal, Honorary, and Foreign Fellows, at the commencement of 1848 was 768. I am not able to say how many of these were Fellows elected in the ordinary way, and how

many were privileged, but this has no importance for my present object. From 1860 onwards the distinction between the three classes, those elected before 1848, Privileged Fellows, and Ordinary Fellows, is exhibited.

At the end of 1890, the total number of Fellows, excluding the Royal, Honorary, and Foreign Classes, was 463; of whom 26 were Fellows elected before 1848; 36 were Privileged Fellows elected since 1848; and 401 Ordinary Fellows elected since 1848.

Hence it appears that the reduction of number of Fellows, of the three classes last referred to, has been 305, and as the number of admissions of the Privileged class has not been very materially affected by the changes in the rules relating to them, it follows that virtually the whole of this large reduction is a consequence of the restriction, to 15, of the number of Ordinary Fellows elected yearly.

As the ages of the 768 Fellows who constituted the bulk of the Society in 1848 are not known, and as the conditions of election before that year differed materially from what they have been since, no very useful conclusions can be drawn from the rate of their diminution since 1848.

Assuming, however, that the number of Privileged Fellows in 1848 was, as is probable, about 50, there would remain 718 Ordinary Fellows, of whom in 43 years 692 lapsed, or at an average yearly rate of 2·24 per cent., that is rather more than 16 a year. This rate, as I shall show subsequently, does not differ greatly from that which has prevailed among the Ordinary Fellows elected since 1848, and it may therefore be presumed that the average age of the Fellows in that year did not differ greatly from the average age since.

Table II gives, as far as available data admit, the ages at the time of election of all Fellows elected since 1848; and shows the number of years they severally survived, the average age at election, the number and average age of those who were alive in 1891, and the greatest and least ages of Fellows elected in each year.

From this table it will be seen that there has been a gradual small increase in the age at election; the average for the first 10 years having been 42·2; for the second 10 years, 43·0; for the third 10 years, 44·8; and for the last 13 years, 45·2.

The accuracy of these conclusions may be somewhat affected by the greater number of unknown ages in the earlier years, the age when unknown, having been taken at the average of the group of years in which the election took place.

The least age at which any Fellow has been elected is 24, one such case being recorded. The average minimum at any election is slightly under 30, and the average maximum is rather over 63, one election at an age of 87 is recorded, and several above 70.

The oldest survivor of the Fellows elected since 1848, who alone are dealt with in this table, was 86 years of age in 1891.

The average age at election was 43·9, and the average age of all the Fellows in 1891 was 58·4.

Table III records the numbers of Ordinary Fellows elected in each year, and remaining alive in each year after election, until 1891.

From this it will be seen that during the last ten years the numbers have increased by 46; in the previous ten years the increase was 68, or 22 more; and in the ten years still earlier the increase was 111, or 43 more than the last. If the decrease of growth for the ten years after 1890, takes place in a similar ratio to that which took place between 1870–80 and 1880–90, we might anticipate an increase of only 11 up to 1,900, or probably a smaller number.

In order to obtain a satisfactory comparison between the lives of the Fellows, and those of the general population as shown in the accepted life tables, I have calculated, from the known ages of the Fellows at election, and the known dates of the deaths that have occurred among them, the average age of the Fellows remaining alive in each year. From these ages I have computed, from Dr. Farr's tables, the probable number of Fellows that would survive from year to year, assuming the initial number to be 15.

From Table III, above referred to, have been ascertained, the number of Fellows surviving in each successive year after election, and thence has been obtained the average number surviving from an initial number 15.

The results of these computations will be found in Table IV.

The second column in this table shows the number of lives dealt with for each year after election. The first entry 645, is the total number of Fellows elected in the whole 43 years. The next column to the right gives their aggregate ages, and the next their average age 44·9, in their first year. Following the same line to the right, we find the average number of Fellows elected, and in their first year.

Passing to the second line of the table, 619, immediately below 645, is the total number of Fellows remaining in their second year from the elections of 42 years; this is succeeded, in the columns to the right, by their aggregate ages in their second year, and their average age, and the average number in their second year, out of 15, the average number elected.

The third line gives the same data for the third year of Fellowship, and so on throughout, the last line but one showing that in their 42nd year there remained 6 Fellows from the elections of 2 years, with an aggregate age of 444 years, and an average age of 74·0; the average number surviving in their 42nd year, out of the 15 elected, being 3.

The sixth column of the table gives the successive sums of the

numbers in the fifth column, and therefore indicates the aggregate number of Fellows that will, on the average, be surviving in each successive year of Fellowship, the number elected in each year being always supposed to be 15.

It will be seen that the total for the 43rd year is 397·0, whereas the actual number surviving, shown in column XI, is 401. This difference is of course due to the number 397, representing what the result would be if the average rates of election and decrease prevailed, instead of the actual rates for the separate years; and it is probably sufficiently accounted for by the fact already pointed out, of the gradually increasing age at election in the later years, which will lead to the lives in the earlier years of the series being somewhat better than the average. Column XI shows the *actual* results for successive years corresponding to the average results given in column VI. The differences will be seen to be somewhat irregular, but nowhere to be of importance.

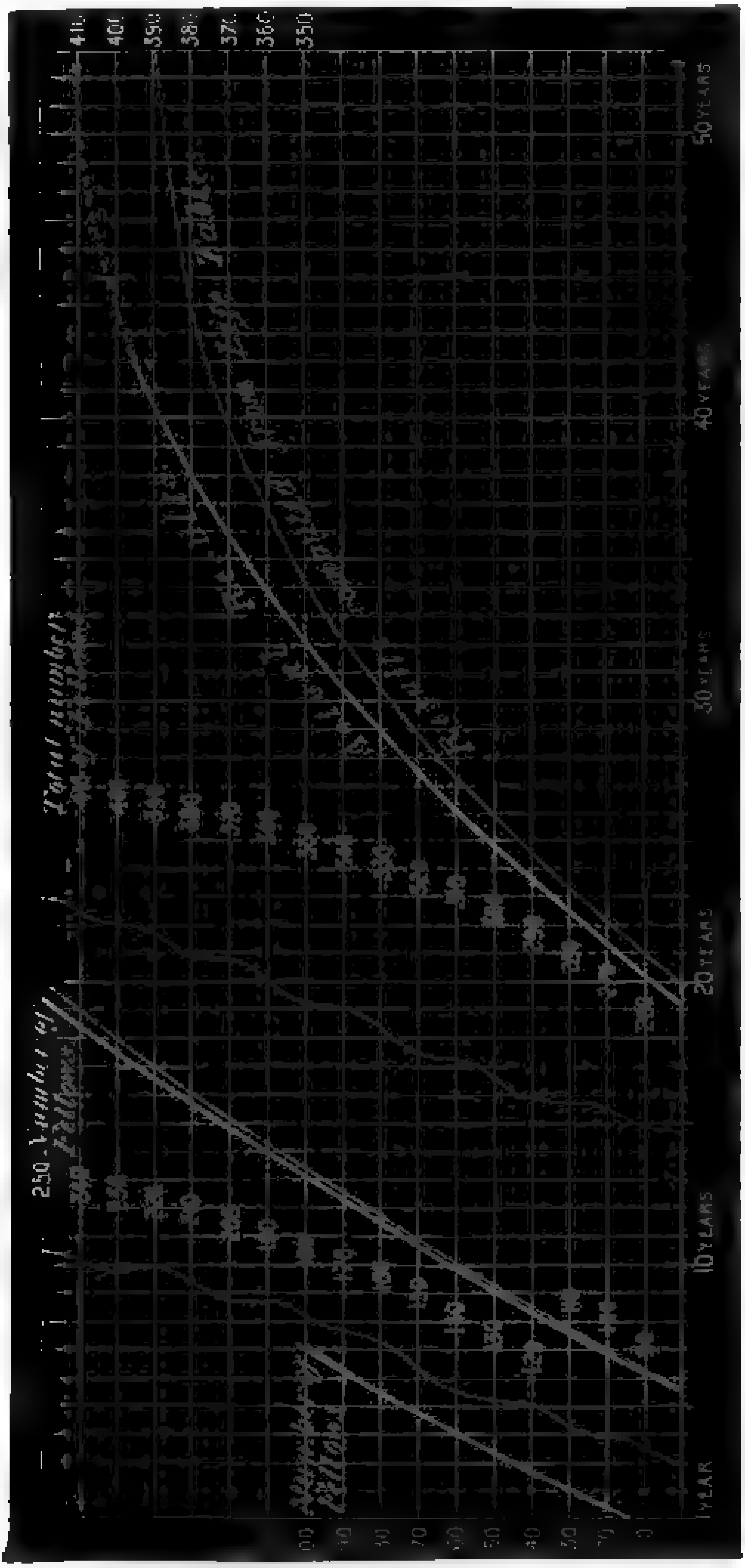
Column VII gives the aggregate ages of the numbers surviving in successive years, as shown in column V, and from it is deduced the average age of the whole number of Fellows shown in column VI, 397, which is seen to be 57·7 years, a result differing slightly from that obtained from the actual ages of the Fellows surviving in 1891, which was shown to be 58·4. The cause of this difference has already been indicated.

Columns VIII and IX supply the results that would be obtained by applying to an initial number of 15, the rates of mortality in Dr. Farr's tables, for the ages in successive years given in column IV. Column X contains the ratio of column VI to column IX, and indicates that throughout the whole period of 43 years the actual results are somewhat better than the tabular results, or that the lives of the Fellows are better than the ordinary lives, and that this advantage leads in the 43rd year to the actual number of survivors being rather more than 5 per cent. in excess of that which would be given by the life tables, or of about 20 on a total of 400.

An examination of this table will show that with the exception of the last six or eight years, in which the number of lives dealt with at last becomes very small, the figures indicate a very regular and consistent progression, and it will practically be quite safe to assume that the series in column VI may be extended on the basis of the ordinary life tables, subject to the addition of 5 per cent. on the total amounts obtained from these last.

Hence it will be found that in 10 years after 1891 the aggregate number of Fellows is not at all likely to be increased by more than 15, that the final result may be as little as 410, but is not likely to be more than 420, or at the outside 425.

The results of this enquiry are shown graphically in the accom-



Graphical representation of chief results given in Table IV.

panying diagram, in which the thick line shows the progressive series of actual numbers contained in column VI, and the thin line those in column IX derived from the life tables. The dotted prolongation of the thick line in a direction conformable to that of the thin line, will be seen almost necessarily to fall below the horizontal line indicating a total strength of 420.

In an earlier part of this paper I mentioned that the rate of decrease of the Ordinary Fellows elected before 1848 did not appear to differ materially from that which has prevailed subsequently.

Taking the number of Ordinary Fellows elected before 1848, and then alive, at 718, it will be found that in 12 years (1860) the number was reduced to 422, which is about 60 per cent. of the original number; after 24 years (1872) the number fell to 206, which is about 30 per cent. of the original; and in 36 years (1884) there remained only 65, which is about 9 per cent. of the first number.

Assuming that the average age of the 718 Fellows elected before 1848, and then alive, was not materially different from the average age (58) of the Fellows elected after 1848 and alive in 1891, when it has probably become nearly stationary, it may be inferred that the lapses among a body of Fellows of that age will correspond to the lapses among the Fellows alive in 1848. Now, from Table IV it will be seen that of the Fellows elected after 1848, the average age in their 17th year was 58·3 years, which is almost exactly the average age of the whole body. Further, it is shown that of the supposed original 15 there remained 10·9 in the 17th year, of the age above mentioned, 58·3. This number was reduced in 12 years to 6·7, which is nearly 60 per cent. of the number in the 17th year, and again falls after 12 years more to 3·7, which is not very different from 30 per cent. of the starting number, and after 12 years more the number will be seen to be likely to be less than 1·0, which again will not differ materially from 9 per cent. of the original 10·9. These proportions, it will have been observed, are those above shown to hold in the case of the Fellows elected before 1848.

On the whole it seems to be established that the present restriction to 15 of the number of Ordinary Fellows elected in any year will lead to an eventual maximum number not exceeding 420; and that the ultimate increase of the total strength of the Society, for each additional Fellow elected in excess of 15 may be taken at 28, so that an increase of the annual number of Ordinary Fellows elected to 18 would lead to an ultimate total of 500 such Fellows.

Table I.—Summary of Numbers of Fellows of the several classes remaining in the Society in each year from 1848 to 1891.

Year.	Total at the commencement of year.	Privileged Fellows.	Fellows elected before 1848.	Ordinary Fellows elected from 1848.
1848	768	Not separable.	768	
9	751		737	14
1850	748		719	29
1	736		692	44
2	720		661	59
3	707		636	71
4	701		616	85
5	688		588	100
6	671		556	115
7	661		535	126
8	658		517	141
9	647		495	152
1860	637	51	422	164
1	621	54	391	176
2	607	51	368	188
3	606	49	356	201
4	602	50	338	214
5	599	49	324	226
6	586	49	300	237
7	572	42	281	249
8	561	40	267	257
9	548	35	247	266
1870	544	36	229	279
1	544	38	219	287
2	542	38	206	298
3	535	38	197	300
4	524	38	177	309
5	525	38	166	321
6	515	42	148	325
7	511	42	136	333
8	505	43	122	340
9	501	42	116	343
1880	488	42	101	345
1	486	42	89	355
2	480	40	82	358
3	477	43	71	363
4	473	41	65	367
5	468	38	60	370
6	465	36	55	374
7	464	34	49	381
8	465	33	45	387
9	469	36	38	395
1890	466	37	32	397
1891	463	36	26	401

…e years 1848 and 1890.

Fellow… electe… Yea… of electi…	15. Age.	15. Life.	16. Age.	16. Life.	Average age at election.	Average age of survivors in 1891.	Number of survivors in 1891.	10 years' mean of age at election.	Least age at election.	Greatest age at election.
184…	..	..			42	71	2		26	65
	42	3			40	77	3		30	50
185…	42	3			38	77	4		29	49
	42	1			37	72	6		26	53
	42	1			42	73	4	42·2	27	63
	53	12	58	10	43	70	4		26	58
	54	6			44	76	4		36	58
	61	3			46	77	5		28	62
	52	2			47	86	1		36	64
	33	1			42	70	6		29	63
	43	6			39	68	7		24	49
	..	..			43	67	6		28	67
186…	45	1			46	70	6		32	66
	43	9			40	66	6		29	56
	43	5			43	69	4		32	63
	43	14			39	65	8	43·0	30	54

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Table IV.—Sho

Successive years of fellowship.	Numb survi in e yea
I.	II.
1	645
2	619
3	600
4	575
5	557
6	536
7	512
8	493
9	472
10	[illegible]
1	427
2	407
3	387
4	367
5	340
6	315
7	295
8	277
9	255
20	239
1	220
2	204
3	186
4	171
5	154
6	141
7	128
8	113
9	100
30	92
1	81
2	72
3	62
4	[illegible]

III. "On the Shoulder Girdle in Ichthyosauria and Sauropterygia." By J. W. Hulke, F.R.S. Received April 11, 1892.

(Abstract.)

The author discusses the structure of the shoulder girdle and the homologies of its several parts in these families. He shows that the alleged existence of a precoracoid in the Ichthyosauria rests on an insufficient foundation; offers proofs that in Plesiosauria the anterior ventral ray is not only theoretically but *actually* precoracoid; and also that the dorsal ray in the girdle *is* homologous with the shoulder-blade in Testudinata and other Reptilia.

IV. "On the Embryology of *Angiopteris evecta*, Hofm." By J. Bretland Farmer, M.A., Fellow of Magdalen College, Oxford. Communicated by S. H. Vines, M.A., F.R.S. Received March 28, 1892.

During a recent visit to Ceylon I took the opportunity of collecting young plants and prothallia of *Angiopteris evecta*, with the view of working out the embryology of this type of the eusporangiate Filicineæ, since the development of the embryo is not as yet known in any member of this group.

Most of my specimens were obtained from clay banks in the vicinity of Peradeniya, where the plants are not uncommon. The prothallia are easily distinguished from those of other Ferns by their somewhat orbicular shape, with crenate edges, as well by their strikingly deep-green colour. They resemble the thalli of *Anthoceros* rather than a common Polypodiaceous prothallium, and indeed are not easily distinguished from the former when the two plants are associated on the same bank, as is frequently the case.

The germination of the spore and the development of the prothallium have been described by Jonkman,* who also observed the formation of the sexual organs. The antheridium is formed from a superficial cell of the prothallium, which divides by a wall, parallel to the surface, into an outer shallow cell and an inner cubical cell. The former, by walls at right angles to the free surface, gives rise to the cover cells; while the inner one, by successive bipartitions, originates the antherozoid mother-cells. This Fern is a very favourable type for exhibiting the development of the antherozoids from the nucleus of the spermatocyte, on account of the relatively large size of the structures in question.

* 'De geslachtsgeneratie der Marattiaceeën,' door H. F. Jonkman.

The antheridia are distributed both on the upper and lower surfaces of the prothallium, and apparently without any approach to regularity, though they are somewhat more frequent on the lower surface. I may observe, however, that an antheridium may often occur on the upper surface immediately above an archegonium which has been fertilised.

The archegonia occur exclusively on the lower surface. Their development has been described by Jonkman, who also noticed the division of the neck canal cell, by a transverse wall, into two cells. The division is not, however, invariable, and in one preparation in which the protoplasm had shrunk slightly from the wall, I observed that the cell plate had not extended so as to completely partition the neck passage into two cells.

The neck canal, and ventral canal, cells become converted into mucilage, which bursts open the archegonium, and thus admits of the passage of the antherozoid to the oosphere.

The oospore, after fertilisation, speedily forms an ovoid cellular body, and although I was not so fortunate, owing to scarcity of material, as to see the formation of the earliest cell walls, their succession could be determined with tolerable certainty in the youngest embryo that I met with, consisting as it did of about ten cells.

The basal wall is formed, as in *Isoëtes*, at right angles to the axis of the archegonium. The next one in order of occurrence I believe to be the median wall, which can easily be distinguished, even in advanced embryos, as a well-defined vertical line.

The transverse wall is much more indefinite, and early loses its individuality owing to the unequal growth of the various parts of the young embryo. The further cell-division is irregular, and to a far greater extent than is the case with the leptosporangiate Ferns as described by Hofmeister and Leitgeb. I was unable to determine the constant occurrence of segment walls, though indications of them could occasionally be seen in a few preparations.

The anterior epibasal octants together give rise to the cotyledon; the stem-apex is formed, not as in the leptosporangiate Ferns, from *one* octant only, but from *both* of the posterior epibasal octants, though one of them contributes the greater portion. The truth of this statement is seen on examining vertical sections through the embryo cut at right angles to the median wall, when a few cells on each side are seen to be clearly marked out by their dense protoplasmic contents and large nuclei, as meristem cells. There is no single apical cell in *Angiopteris* from which all the later stem tissue is derived, and this fact is, without doubt, to be connected with the character of the apical meristem just described. The root is formed from one of the octants beneath the cotyledon, *i.e.*, from an anterior *hypobasal* one, and is at first indicated by a triangular apical cell,

which, in one fortunate preparation, showed the first cap cell. The other octant, together with the two posterior hypobasal octants (which together form the rudimentary foot), round off the base of the embryo. The root presents considerable difficulty in tracing the course of its development, as the apical cell, at no time very clear, is early replaced by two cells, as I convinced myself by an examination of sections specially cut obliquely in order to determine this point. Moreover, the root grows in a somewhat sinuous manner in the embryo, and the cells of its apex may easily be confounded with other triangular cells which occur irregularly scattered in the lower portion of the embryo. It finally emerges, not immediately beneath, nor yet exactly opposite, the cotyledon, but at a distance from it of between one-third and one-half of the circumference of the embryo. The difficulties attending the exact following of its growth, added to the scarcity of the material, have prevented my elucidating completely the details of development, but the important point, that, even before its emergence from the embryo, its apex contains a *group* of initial cells occupying the place of the single one characteristic of other orders of Ferns, can be regarded as established with certainty.

The vascular strand, which is differentiated early in the cotyledon, joins on to that of the root, and the first tracheid appears in that part of the bundle which is opposite to the junction of the cotyledon with the stem, in fact, just at the point where the leaf-trace curves round into the latter. Thence the further differentiation of the xylem takes place in an upward and downward direction.

When the embryo has reached a certain size it bursts through the prothallium, the root boring through below, whilst the cotyledon and stem grow through the upper surface. This manner of issuing from the prothallium at once serves to distinguish *Angiopteris* from those other Ferns whose embryogeny is known, and probably the peculiarity of its growth may be reasonably connected with the direction and position of the basal wall which separates the root and short portions of the embryo. It will be remembered that in this plant the basal wall is parallel to the plane of the prothallium, instead of nearly at right angles to it, as in the leptosporangiate Ferns.

Fresh leaves and roots speedily arise on the young plantlet, the second leaf appearing just above the place of exit of the first root, that is, not quite opposite the first leaf. The third leaf rises between the first and second ones, and nearer the first than the second. Their roots observe the same rule of divergence as that which obtains in the case of the first root. The stipular structures, so characteristic of the Marattiaceæ, are entirely absent from the first two leaves, but appear in a well-developed condition on the third and all succeeding leaves. They fulfil the function of enclos-

ing and protecting the younger foliar structures, from the time of their first formation.

V. "Note on Excretion in Sponges." By George Bidder. Communicated by Adam Sedgwick, F.R.S. Received April 9, 1892.

In a review* of Mr. Dendy's work on the Homocœla, I briefly described (p. 628) the "flask-shaped" or "glandular" epithelium, which I believe to form the most common external covering in all groups of sponges. On p. 631 are shortly mentioned certain other granular cells, believed by Metschnikoff to be mesodermal, and by Dendy to be the dwelling place of symbiotic Algæ; I proposed the neutral name of "Metschnikoff cells." "In *Ascetta clathrus* there is an additional point of interest, that the granules in the (glandular) ectoderm cells differ from these," *i.e.*, the granules in the Metschnikoff cells, "only in being of smaller size. I have been very slowly and gradually led to the conclusion that the bodies in question, which I propose to call 'Metschnikoff cells,' are metamorphosed collar cells; that by their reaching to the exterior and becoming perforated, pores are formed; and that the granules of these and of the ectoderm, and of the glandular ectoderm in general (and possibly the granular cells so frequently described beneath it in Silicea), are excretory."

This latter proposition, so far as concerns *Ascetta*, may now be considered proved, and I think the observation sufficiently important to justify my asking permission to communicate it to the Society. Leaving a sponge in a solution of indigo-carmine in sea-water (at first I used a saturated, but afterwards a weaker, solution), I found that the granules normally present in the Metschnikoff and ectoderm cells become replaced in part by dark-blue granules, no other part of the sponge being in any way coloured blue. Fig. 1 shows a Metschnikoff cell from a specimen of *Ascetta clathrus*, which had been thirteen hours in saturated indigo-carmine solution. The black dots represent the granules which were blue, the colourless circles those which were of the usual yellow; a few of intermediate shading represent granules which appeared pale-blue or green. Focussing showed that, while this cell stretched under the spicule into the deep parts of the sponge wall, the left-hand extremity emerged on the upper (ectodermal) surface.

This particular cell I had the pleasure of demonstrating, while

* 'Quart. Jl. Micr. Sci.,' Oct., 1891, *Review*—Subsequently reprinted, by the permission of Messrs. Churchill, under the title of 'Notes on Calcareous Sponges.'

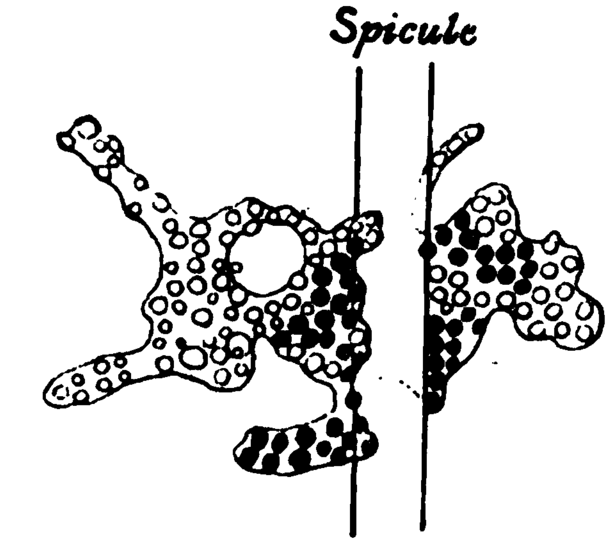

FIG. 1.—Living Metschnikoff cell, focussed through the ectoderm. Some of its granules are of yellow pigment, and some of the indigo which has been administered.

living, to many gentlemen working in the Zoological Station. I have also successfully repeated the experiment several times, and (by the use of concentrated corrosive sublimate with picrocarmine in sea-water, followed by anhydrous alcohol and chloroform) have succeeded in making permanent preparations and sections showing the blue indigo granules, the nuclei stained red, and, though somewhat changed, the original pigment granules.

In a Homocœl sponge found at Naples, corresponding with the ordinary diagnosis of *Ascetta primordialis*, I found, after twenty-eight hours in indigo-carmine, that while neither the endoderm nor the "mesodermal" tissue showed any tinge of blue, the flask-shaped ectoderm cells were coloured faintly so, and the granules contained in them strongly blue. The nuclei were colourless, thus excluding the hypothesis of *post-mortem* staining, under which they colour darkly.

I have not yet observed with certainty the excretion of indigo-carmine by the ectoderm cells of *Ascetta clathrus;* the conditions under which excretion is performed respectively by Metschnikoff cells or ectoderm cells remain, therefore, still to be defined. But in this sponge the appearance of the yellow granules in the two kinds of cells is precisely the same, except that those in the Metschnikoff cells are larger, being about $1 \cdot 7\mu$ in average diameter, as opposed to $1 \cdot 1\mu$. And of many characteristic reactions, such as extreme solubility in distilled water, alkaline solutions, alcohol, and other fluids; solubility in dilute acetic acid, and insolubility in sea-water; a "port wine" colour, disappearing on heating, with iodine in sea-water, and a nearly black colour with osmic acid, I have found no one which differentiated the granules in the two kinds of cell.*

All the forms shown by the Metschnikoff cells appear to be strongly

* With reference to Topsent's observations ('Clionidæ,' 1888, p. 48) on *Cliona*, it is worth mentioning that in no part of *A. clathrus* is there any trace of a blue reaction with sulphuric acid.

suggestive of the hypothesis that they are loci of discharge, first into the gastral cavity, subsequently on the exterior surface. To this they have obviously pushed their way (*cf.* fig. 2), possibly in

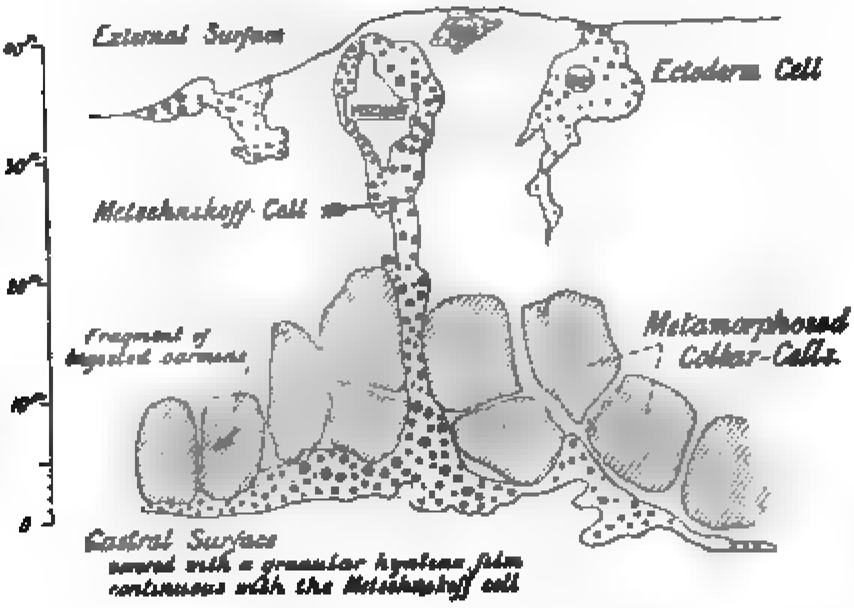

FIG. 2.—Section through the wall of *Ascetta clathrus*. Osmic acid. Nuclei not stained.

consequence of the occlusion of the gastral cavity. For the flask-shaped ectoderm cells, their whole form and disposition indicate with certainty that they are glands discharging to the exterior.

A completely different set of granules, which must not be confused with these, are found in the normal collar cells of many sponges; for distinction it may be well to speak of them as "basal spherules of the endoderm," or simply "spherules." I have mainly experimented on them in *Sycon raphanus*, *Ascaltis cerebrum*, and *Ascetta primordialis* (so-called). In the fawn-coloured varieties of these two latter species, the basal spherules give the colour to the sponge; in the living *S. raphanus* they are of a greenish tint, more refractive than the protoplasm, but not highly so.* They never blacken with osmic acid, nor give a port wine reaction with iodine, nor do they show any coloration in a sponge which is excreting indigo (*A. primordialis*).

In the fresh condition they stain (*S. raphanus*) more deeply than the nucleus with acetic acid and methyl-green, and with osmic acid and methyl-green, but they are not stained red by picrocarmine after osmic acid. In permanent preparations they appear with osmic acid

* Similar greenish granules were observed in *Spongilla* by Lieberkühn in 1856 and Carter in 1857. Saville Kent ('Manual of Infusoria,' p. 80) considers the colour to represent "the predominating refractive index of abnormally minute protoplasmic bodies" on grounds which at least point to its frequent occurrence. Similarly disposed granules, but of pink, violet, or other colour, have been frequently observed in the collar cells of sponges.

slightly darker than the surrounding protoplasm, and are generally stained to a greater or less degree by hæmatoxylin.

Their most important reaction, in which they differ alike from the nuclei and the excretory granules, is the characteristically staining during life deeply and immediately with Bismarck-brown. This was observed in *Ascetta primordialis*, *Ascaltis cerebrum*, *Ascandra Lieberkühni*, and *Sycon raphanus*; after keeping *Sycon raphanus* some days alive, but in unfavourable circumstances, the spherules, though still visible, did not stain more than the surrounding protoplasm.

I believe these basal spherules to be stores of nutritious matter. In *Leucandra aspera* and *Sycon raphanus* (I have once observed indications in *Ascetta primordialis*) the collar cell, after it has accumulated a certain quantity of spherules in its base, splits off this base by a transverse fission as a non-nucleated mass of protoplasm, which we may term a "plinth" (fig. 4); the plinth then lies between the

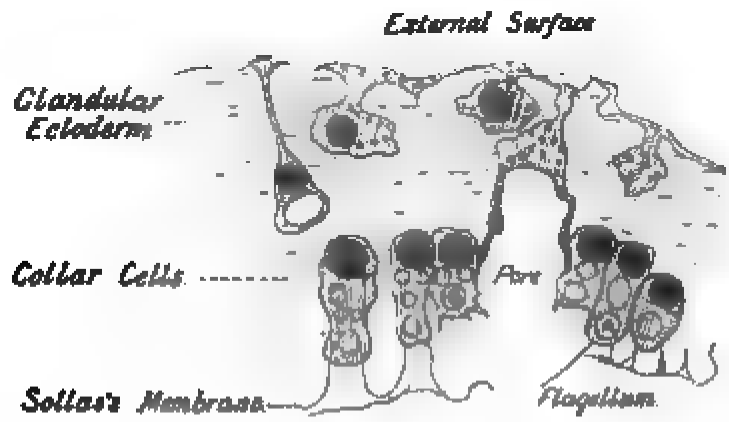

FIG. 3.—Section through the greater part of the length of an afferent pore in *Ascetta clathrus*. The nuclei are shown, and endodermal vacuoles, two of which contain amorphous matter. The spaces left by spicules in the intercellular jelly are not shown here or in Fig. 2. Diameter of pore 6·5μ. Osmic acid and Mayer's carmine.

nucleated distal part of the cell—the "column"—and the mesodermal jelly in *S. raphanus*, or the thin basement membrane which is all that usually divides two epithelia in *L. aspera*. I have observed in the mesoderm of *S. raphanus* large wandering cells, which I believe to be generative elements, with pseudopodia attached to these plinths, and spherules of the same character as the basal spherules both in the wandering cells and in their pseudopodia; there can be little doubt that they were feeding on the reserve stores of the collar cells. The division into column and plinth takes place as a rule at the same time in all or most of the cells of a chamber. The "columns" or distal parts appear as small, columnar, nucleated cells, provided with a small amount of clear protoplasm, rudimentary collars not united, and flagella (fig. 4A).

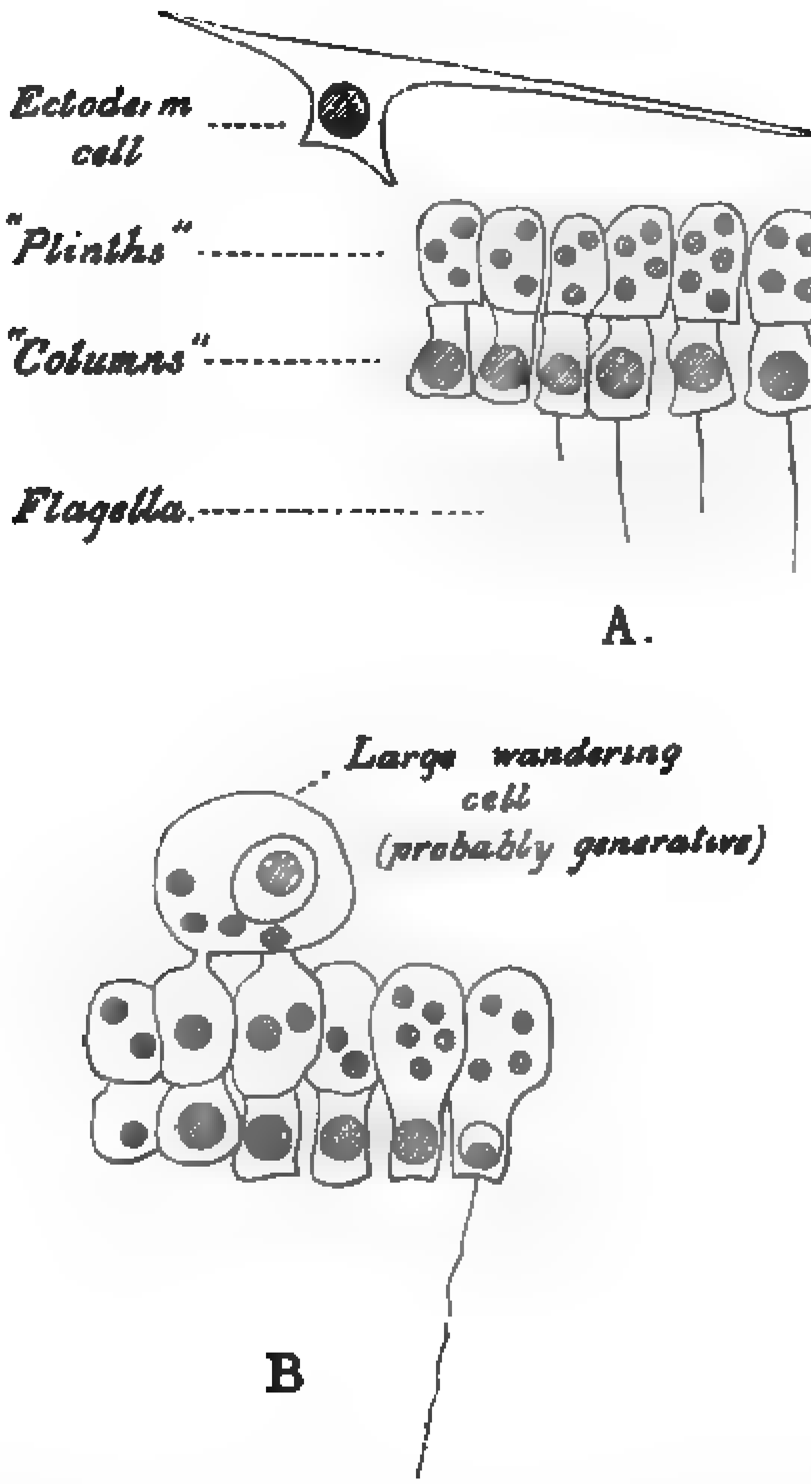

FIG. 4.—Basal spherules of collar cells, *Sycon raphanus*. At A the typical plinth and column is shown. At B (from the same preparation) the fission is less typical, but a large ovum or sperm cell is feeding on the spherules. There are no collars, but many cells show sharply flat, or even concave tops, suggesting that the collars are being formed anew. Osmic acid and hæmatoxylin.

My conception of the metamorphoses of the collar cells is as follows:—

In Heterocœla (probably it is similar in Silicea) the collars of the collar cells are at first mere fringes, which help to retain the food and filter the water as it passes from the base of the cell to the moving tip of the flagellum. When the cell is satiated the flagellum ceases to move, and degenerates; the collar unites with

the neighbouring collars to prevent the water that is already filtered and already foul from returning past the inactive area to pollute the afferent water supply. When the food has been digested, the cells elongate and become closely pressed together; the separation of their basal parts takes place in the manner already described, and the distal parts start on a new cycle with hungry protoplasm, active flagella, and separated collars.

In the Homocœla I have been unable to make sure of these changes other than as regards the habitual association of Sollas's membrane with the absence of flagella, and of the presence of flagella with separated collars. (This was noticed by Sollas, 1888, in the Tetractinellida of the 'Challenger.')

The nucleus in the collar cells of the Homocœla is generally basal, whereas in the Heterocœla, contrary to the current statements, it is almost always distal, the basal spherules having been commonly mistaken for it. The idea rather suggests itself that in sponges with basal nuclei in the collar cells the fission into column and plinth does not take place.

Of the Metschnikoff cells in *A. clathrus* the following is the history. Under certain circumstances the Sollas's membrane and the gastral surface of the collar cells become replaced by a continuous hyaline film charged with the yellow granules characteristic of the sponge (*vide* fig. 2). Certain collar cells—or, rather, quondam collar cells—become charged throughout their whole substance with these granules; they are now Metschnikoff cells, and are the cells described (p. 360) and figured by that author as mesoderm cells in his well-known paper ("Spong. Studien;" 'Zeits. wiss. Zool.,' vol. 32). They are almost always excavated by a cavity or duct,* frequently taking the form of a capillary and sometimes branching tube; they push through towards the ectodermal surface, with which they become connected; the granular film covering the general gastral surface disappears. It is now to be noted that the afferent pores of all the Homocœla I have examined consist each of a single, nucleate, perforated cell;† the film of protoplasm, though very thin compared with the size of the lumen, is charged with granules similar to those found in the ectoderm cells. A longitudinal section through part of

* [*May* 16.—"In the next stage, which I regard as a more advanced condition, they form a ring surrounding an empty discoid or sometimes crescentic space." (Dendy, Monograph, p. 18.)—The "peculiar structures" met with by this author in *Grantia labyrinthica* (p. 17), I interpret as column-and-plinth chambers violently contracted in alcohol; the lateral coherence of the cells renders them subject to this. The "nerve-cells" in fig. 31 of the same paper are my "glandular epithelium." The "gland-cells" under the gastral surface in fig. 26 I believe in *S. raphanus* to be sperm mother-cells, having the remarkable peculiarity that the spermatozoa come to maturity singly.]

† *Cf.* Minchin, 'Quart. Jl. Micr. Sci.,' vol. 33, Pl. 11, fig. 21.

a pore in *Ascetta clathrus* is shown in fig. 3. My conclusion on first detecting the form of the normal ectoderm was that the pores were ectoderm cells which had placed themselves in communication with the gastral cavity. I have found, however, where the conditions are most abnormal the ectoderm cells in their usual flask-shaped form apparently inactive; while, on the other hand, Metschnikoff cells are very frequent in all stages up to the condition in which they are exposed on both gastral and external surfaces and perforated nearly from end to end. I believe that when they have reached this stage they discharge their granules from the external as well as from the gastral surface. The lumen is thus completed, and a perforation through the wall is formed, which, on regeneration of the gastral epithelium, persists as an afferent pore.

The prosopyle cells of the Heterocœla consist also each of a single perforated, nucleate cell, frequently containing granules (*cf.* also Poléjaeff, 'Chall. Calcarea,' Plate III), and I suggest the hypothesis that primitively the afferent pores of sponges are perforated excretory cells derived from the endoderm, while the ectoderm is a layer of cells excreting constantly from the intercellular jelly, their flask-shaped form having been developed to expose the greatest possible surface to the medium from which the excreted substance is derived. They have been differentiated on the exterior as a covering to the nutritory and reproductory cells of the sponge, in order, by reason of their noxious contents, to form some protection to the naked protoplasm.

[*May* 16, 1892.—I wish to suggest, as a proper subject for expert chemical analysis, the question, how far the substance excreted by Calcarea in the granules of the ectoderm and Metschnikoff cells is allied to the so-called spongin.

At first sight, such a proposition may appear idle. But Kölliker, O. Schmidt, and many others have recorded in horny sponges cuticle of various degrees of insolubility, up to a point at which it cannot be distinguished in constitution from the horny skeleton with which it stands in connexion. Both from my own observations on an *Aplysilla* (?) at Naples (bearing a cuticle insoluble in caustic ammonia), and from a study of Schulze's detailed description of the superficial structure, particularly in *Euspongia*, I am persuaded that the ectoderm cells of the horny sponges are of the same form and character as those in the Homocœla. Again, the slimy covering of *Halisarca* (Merejkovsky) is precisely homologous with the granules excreted by the ectoderm of calcareous sponges. If the cuticle of horny sponges be granular—as Schmidt found it in *Esperia tunicata*—these three products are morphologically identical, and it is not unreasonable to inquire if they are chemically related.

Further, I cannot find the adequate arguments that have led to the

abandonment of the old view, that the horny fibres are derived from the cuticle and the ectoderm; I can see no evidence advanced for the mesodermal nature of the "cap-like investment belonging to the dermal layer of the connective tissue" which Schulze finds ('Spongidæ,' 1879, p. 637*) to be the organ productive of the principal growth of the fibre; I think that Lendenfeld is probably right in his homology of the spongoblasts with gland cells (1883, 'Zeitschr. wiss. Zool.,' vol. 38, p. 256), and in his statement that the two tissues are continuous one with another ('Horny Sponges,' 1889, p. 790). Having found with certainty that the flat epithelium, which he draws and describes in detail ('Monograph of Australian Sponges,' 1884, 1885, *passim;* 'Monograph of Horny Sponges,' 1889, p. 775; 'Kalkschwämme,' 1891, p. 162), over the gland cells of calcareous sponges has no existence, I reject entirely his evidence that a similar epithelium covers the gland cells of Ceratosa; the more so as Schulze, Poléjaeff, and other conscientious workers have failed to demonstrate it. I hold that these gland cells are the ordinary flask-shaped epithelium which forms the external covering of most sponges, and is presumably the ectoderm. I hold it on the present evidence probable that the spongoblasts are homologous with them, the horny fibre with the horny cuticle, and the hexagonal markings of the horny fibre with the well known "silver lines" of the outer surface.

It is impossible for any unskilled person to attempt the chemical comparison of such substances; the more so as "spongin" is a body —or a series of bodies—hitherto little investigated and vaguely defined. As used by zoologists, it is a convenient word applied to a number of different nitrogenous substances, not easily soluble, found as supporting structure in a large group of more or less allied sponges; while the skeletons of some genera are easily soluble in caustic potash (Kölliker), and even very dilute caustic potash (Dybowski), in others they withstand it at boiling temperature, and in any strength.† We have assumed these substances to be related because of their homologous occurrence in allied animals; I wish to extend this assumption, for what it is worth, to the excretory granules of Calcarea. It is noticeable that, like "spongin," the matter composing these granules is characterised by a tendency to great variation.

* It is just to our leader to say that in the same paper he expressly states that he speaks of "connective tissue," because its homology with "mesoderm" is only probable, and not proved (p. 625). But I suggest that, from his description, it is open to the reader to believe that the cap belongs to the "outer cell-layer, and not to the connective tissue layer." Compare especially p. 626.

† Vosmaer points out that analyses have hitherto only been made on *Eusponjia*. The observations of Ridley, to which Vosmaer gives his support, show that the horny skeletons of some Ceratosa are not doubly refractive.

This variation takes place, not only among closely allied species, but in the granules of one and the same cell; suggesting most strongly that the substance itself is mutable, and that the different aspects it presents are phases through all of which individual granules may pass. Adding distilled water under the microscope to a fragment of white *Ascetta primordialis*, all the granules turn orange-yellow (their colour in the red variety), and the greater part of them can be seen instantaneously to dissolve. But in many of the cells there are a certain number of granules which remain after a week's maceration, and then exhibit considerable resistance to caustic potash; in *Ascandra reticulum* the same phenomena occur with but slight variation.* In the light of this history we may perhaps reconcile, on the one hand, *Ascetta clathrus*, in which all the granules appear to be ultimately dissolved in distilled water, and, on the other hand, *Leucosolenia cavata*, in which Dendy finds them insoluble in boiling caustic potash.

I suggest as a working hypothesis that the yellow granules of *Ascetta clathrus* are a soluble nitrogenous excretion which is highly mutable, and readily gives rise to the less soluble substances occurring with it in the granules of other Homocœla. I suggest that this substance is probably the common nitrogenous secretion of the protoplasm in all sponges, and that the horny sponges are those which have learned to retain it within their bodies until it has formed one or other of those more or less insoluble products which we usually recognise under the hospitable name of spongin. I hope soon to discuss this hypothesis more at length, and to give the full grounds for my conviction as to the general presence in sponges of a glandular or flask-shaped external ectoderm.

The homology between cuticle and horn fibres has been insisted on by Kölliker, O. Schmidt, Hyatt, and others; the resemblance between dermal cells and spongoblasts has been noted by Marenzeller, and emphasised by Lendenfeld; their complete homology, and the probability of the slime of the skin being "spongin and much water,"

* If either of these two sponges be shaken violently for three minutes in distilled water, and the liquid thoroughly filtered, a clear yellow (golden-yellow *A. primordialis*) slightly alkaline solution results. Acidifying with 1 part in 10,000 of acetic acid, there is no immediate effect; but in the course of some hours a flocculent brown precipitate is thrown down, insoluble except on boiling, in either concentrated caustic potash or nitric acid; the solution made with boiling nitric acid leaves a straw-coloured residue on evaporation which turns rich yellow on the addition of ammonia, and becomes black on over-heating. I may note that I have now (May 14) succeeded in obtaining crystals from a solution made by placing an *Ascetta clathrus* in absolute alcohol; after remaining some months in a bottle and decreasing in volume, the alcohol contains a number of long, thin, glittering blade-like crystals, as much as 3 mm. long, doubly refracting, very soluble in water; the nitrate appears to be soluble in nitric acid.

is insisted on by Lendenfeld; the derivation of sponge fibres, ontogenetically, by invagination of the outer surface, has been observed by Barrois and O. Schmidt; the probability that spongin arose as a nitrogenous excretion has been suggested by Eisig.

While I cannot ask space here to acknowledge my nnmerous obligations both at Cambridge and Naples, it is impossible not to express my thanks to Professor Dohrn for his great kindness in placing a table at my disposal in the Zoological Station.]

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Framed copy of Capt. Weir's Azimuth Diagram.

Mr. J. D. Potter.

Photographs of the region of Nova Cygni and Nova Aurigæ.

Mr. Isaac Roberts, F.R.S.

May 19, 1892.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

Dr. George Mercer Dawson (elected 1891) was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

Pursuant to notice, Professor W. Kühne, Professor Éleuthère Élie Nicolas Mascart, Professor Dimitri Ivanovitch Mendeleeff, and Professor Hubert Anson Newton were balloted for and elected Foreign Members of the Society.

The following Papers were read :—

I. "On Nova Aurigæ." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S., and Mrs. HUGGINS. Received May 16, 1892.

[PLATE 4.]

We had the honour in February last of communicating to the Royal Society a short preliminary note on the remarkable spectrum of this temporary star. We beg now to present a fuller account of our observations, together with two maps of the spectrum of this star, and some theoretical suggestions as to its nature. One map represents the result of our work by eye in the visible region; the other map has been drawn from a photograph of its spectrum, taken without its light having passed through glass, and which extends into the ultra-violet nearly as far as the absorption of our atmosphere permits even the solar rays to pass.

On the Visible Region of the Star's Spectrum.

The kindness of Professor Copeland in sending us a special telegram on February 1 enabled us to commence our observations of the star on February 2, when it was of about the 4·5th magnitude. These observations were continued on the following evening, and on the 5th, 6th, 22nd, and 24th February, and on the 15th, 18th, 19th, 20th, and 24th March, when the sky was more or less sufficiently clear for further observations to be made by eye. On the two ends of the spectrum the observations were usually made with a spectroscope containing one dense prism of 60°, but the comparisons in the brighter parts of the spectrum were observed with a more powerful spectroscope containing two compound prisms.

Comparisons with Hydrogen.—Three bright lines of great brilliancy, about the positions H_α, H_β, and H_γ, left little doubt that they were due to hydrogen. The corresponding lines of a hydrogen vacuum tube were found to fall upon these lines, showing that they had their origin in this gas; but the line in the star at F, which could be best observed, showed a large shift of position towards the red. The line from the vacuum tube fell not upon the middle of the line, but near its more refrangible edge. The star line was brighter on the more refrangible side, so much so, indeed, that our first impression was that this side of the line only might be truly H_β, and the less bright part towards the red, a line of some other substance falling near it. Subsequent observations of the hydrogen lines in the star left no doubt that though they presented the unusual character of being double, and sometimes triple, they were due wholly to hydrogen. These lines were rather broad, but defined, especially so at the more refrangible edge. Similarly to what is observed in

the spectrum of terrestrial hydrogen, C was narrower than F, which again was less broad than H_{γ} near G.

The remarkable phenomenon presented itself that all the bright hydrogen lines and some other of the bright lines were doubled by a dark line of absorption of the same gas on the blue side. The shift of the dark hydrogen lines towards the blue showed a velocity of approach of this cooler gas somewhat greater than the recession of the gas emitting the bright lines. Our estimates of the relative velocity would place it at about 550 miles a second, which is in good accordance with the result obtained by Professor Vogel from the measurement of his photographs.

So far as our instruments enabled us to determine the point under the unfavourable condition of the rapidly waning light of the star, no great change in the relative motion of the gases producing the bright and dark lines took place from February 2 to about March 7, when the star's light became too faint for such observations—a result which we believe to be in accordance with successive photographs taken at Potsdam, Cambridge (U.S.), Stonyhurst, and some other observatories.

Comparison with Sodium.—A bright line, which on one occasion we glimpsed as double, appeared about the position of D.

Direct comparisons with a sodium flame, while leaving no doubt that the line was due to this substance, showed that it was shifted, similarly to the bright hydrogen lines, towards the red. Perhaps we should state that at the time we had the impression that this line was not shifted to so large an amount relatively to sodium as was the F line relatively to hydrogen. As the comparison was more difficult at this part of the spectrum, and one prism only was used, we do not attach importance to this observation.

Comparisons with Nitrogen and Lead.—There can be little doubt that one of the four brilliant lines in the green is the same line which appeared in the Nova of 1876, and was at that time suspected to be the chief nebular line. Very great pains were taken to ascertain its exact position and character.

For this purpose, on February 2, and again on February 3, direct comparisons were made with the more powerful spectroscope of the star's line with the brightest double line of the nitrogen spectrum, and also with a line of lead, to which line the near relative position of the nebular line is accurately known. Comparisons on both nights, and with both lines, showed that the star line was certainly less refrangible than the chief nebular line, and by a much larger amount than the shift of F relatively to hydrogen. A similar conclusion has been arrived at by Professor Young, Professor Vogel, Dr. Campbell at the Lick Observatory, Father Sidgreaves, Dr. Becker, and M. Bélopolsky at Pulkova. The position of the line in the star

is about λ 5014, and the line may well be one about this position which is frequently seen bright at the Sun's limb.

It may be added that though three faint bright lines are to be seen in the star's spectrum, not far from the place of the second nebular line, no one of them can be regarded as that line. Indeed no certain evidence exists that the chief nebular line occurs without the second line. In some cases of my early observations on the nebulæ in which I recorded the spectrum as consisting of one line only, I have since with better instruments been able to see the second and the third lines as well. The origin of the second, as well as that of the chief nebular line, is not known. Professor Keeler has shown that the second nebular line is not coincident with the double line of iron, which is very near it.

The conclusion that the spectrum of the Nova has no relationship with that of the bright-line nebulæ would be strengthened, if further confirmation were needed, by the absence in a photograph we took of the spectrum of the New Star of a very strong ultra-violet line which is usually found in the spectrum of the nebula of Orion.

Comparison with the Hydrocarbon Flame and Carbon Oxide.—The brightest line in spectrum of the Nova, with the exception of F, falls near the brightest edge of the green fluting of the hydrocarbon flame. Direct comparisons showed the star line to fall a little to the red side of the edge of the fluting; but, allowing for a shift of the star's spectrum, the place of the line would be near, though not coincident with, the brightest edge of the fluting.

The character of the star line leaves, however, no doubt on this point, for it is multiple with the brightest and most defined line on the blue side, contrary to the fluting which is defined on the red side, and gradually falls off towards the blue. If any uncertainty could be supposed still to remain, it was wholly removed when we found no brightenings in the star's spectrum corresponding to the other flutings of the hydrocarbon flame. A bright band in the blue falls just beyond the fluting in this region. This band may have the same origin as a similar band in certain of the Wolf-Rayet stars.

We conclude that the spectrum of the Nova has no relationship with the usual spectrum of comets.

We found from direct comparison that the different set of flutings characteristic of the carbon oxide spectrum was not represented by any corresponding brightenings in the spectrum of the Nova.

Comparison with Magnesium.—It was not unreasonable to suppose that the star line might have its origin in magnesium, the triple line of which at *b* falls almost at the same place. The comparison showed the stellar line to fall upon the more refrangible pair of the magnesium triplet, and to overlap it slightly on both sides, but rather more on the blue side. Considering that with the resolving power

used the three lines of the triplet were well separated, and that we sought in vain for a similar triplet in the star; and, further, that if the probable shift of the star's spectrum towards the red be taken into account, the star line would fall rather more to the blue side of the more refrangible pair of the triplet, we consider it probable that the star line has some other origin. The stellar line is multiple, but it was found difficult to observe it with a sufficiently narrow slit. A thin and defined bright line was clearly seen at the blue side of the rather broad stellar line, but the remaining and less bright part of the line was not certainly made out, but on one occasion it was more than suspected of consisting of several lines.

We consider the evidence to be against the star line having its origin in magnesium, especially as no correspondingly bright lines were observed in the Nova at the positions of the other strong lines of the spark spectrum of magnesium, nor in our photograph at the position of the strong magnesium triplet a little more refrangible than H.

The third bright line in the green of the Nova which is nearest to F, and the least brilliant of the group of lines in this region, was found to have a wave-length of about λ 4921. A large number of bright lines were seen in the spectrum besides those which have been entered on the map (Plate 4).

The lines only of which we were able to fix the position with approximate accuracy are drawn across the spectrum. The places of the lines drawn partly across the map are from estimations only.

We observed a line a little more refrangible than D, of which the position when corrected for the shift of the spectrum, is at or very near that of D_3. Also a bright line below C, and others between C and D.

On February 2 and February 3 groups of numerous bright lines crowded the spectrum between *b* and D, which were less easily seen as the star waned.

The continuous spectrum extended, when the star was brightest, below C, and as far into the blue as the eye could follow it, at this time to a little distance beyond G.

The visible spectrum of the Nova, and especially the reversal of H and K, and of the complete series of the hydrogen lines in the ultra-violet, together with the probable presence of D_3, suggest strongly a state of things not unlike what we find in the erupted matter at the solar surface. In a photograph of a prominence taken on March 4, 1892, which I have received from M. Deslandres, not only H and K and the complete series of hydrogen lines are reversed, but three bright lines appear beyond, which may be more refrangible members of the same series.*

* [M. Deslandres informs me that his measures of the positions of the three lines

Photograph of the Ultra-Violet part of the Spectrum.—On February 22 and March 9 we took photographs of the star with a mirror of speculum metal and a spectroscope of which the optical part is made of Iceland spar and quartz.

The photograph taken on February 22 with an exposure of $1\frac{3}{4}$ hour surprised us in showing an extension of the star spectrum into the ultra-violet, almost as far as the limit imposed upon the light of celestial bodies by the absorption of our atmosphere.

Not only the hydrogen lines near G and at *h*, but also H and K, together with the complete hydrogen series which appears dark in the white stars, came out bright, each with its corresponding absorption line on the blue side. There are some inequalities of brightness in these lines, especially in the line δ, which is brighter than γ or β, which probably arise from lines of other substances falling upon them. On this night K was followed by an absorption, which was less intense than the absorption at H.

Beyond the hydrogen series the spectrum is rich in bright lines, which, in most cases, are accompanied by lines of absorption. Necessarily, from the long range of spectrum included on the plate, the scale is small, and for this reason, and from the faintness of the more refrangible portion of the spectrum when observed under the measuring microscope, the positions given to the stronger groups, which alone have been inserted in the map, must be regarded as approximate only.

Below the spectrum of the Nova, the spectrum of Sirius has been placed for comparison. The group near the more refrangible limit of the spectrum* has been drawn in. Numerous other lines between this group and the end of the hydrogen series have been detected in our photographs of Sirius, but have not yet been measured with sufficient accuracy to justify us in putting them into the map.

In this map no attempt has been made to show the shift of the spectrum of the Nova. The bright lines in the star have been put at the places of the hydrogen lines.

To the extreme limit of the spectrum a faint continuous spectrum shows itself.

The photograph of March 9, exposed for $1\frac{1}{2}$ hour, was rather faint, as the state of the sky was unfavourable.

The apparently Multiple Character of the Lines.—On February 2 we noticed that the F line was not uniform throughout its breadth, and soon came to the conclusion that it was divided, not quite symmetrically, by a very narrow dark line. The more refrangible component was brighter, and rather broader than the other. Later on in Feb-

fall into Balmer's formula for the hydrogen series. We must regard them, doubtless, as members of that series and due to hydrogen.—June 10.]

* 'Roy. Soc. Proc.,' vol. 48, p. 216.

ruary, we were sure that small alterations were taking place in this line, and that the component on the blue side no longer maintained its superiority. We suspected, indeed, at times that the line was triple, and towards the end of February and in the beginning of March we had no longer any doubt that it was occasionally divided into three bright lines by the incoming of two very narrow dark lines.

Similar alterations, giving a more or less apparent multiple character to the lines, are to be seen not only in the bright lines, but also in those of absorption in contemporary photographs taken of the spectrum of the star. I may mention those taken at Potsdam, Stonyhurst, and the Lick Observatory. They were specially watched and measured by M. Bélopolsky at Pulkova.

Professor Pickering informs me that on a photograph taken at Cambridge, U.S., on February 27, H, K, and α are triple, and that Miss Maury recorded, "the dark hydrogen lines rendered double, and sometimes triple, by the appearance of fine bright threads superposed upon the dark bands."

To explain these appearances on the assumption that each component of the bright and dark lines is produced by the emission or absorption of hydrogen moving with a different velocity would require a complex system of six bodies all moving differently.

A much more reasonable explanation presents itself in the phenomena of reversal, which are very common on the erupted solar surface and in the laboratory.*

Professor Liveing informs me that he and Professor Dewar, in their researches with the arc-crucible, met with cases in which, through the unequal expansion of the bright line on the two sides, the narrow reversed dark line did not fall upon the middle of the broader bright line, but divided it unsymmetrically. This effect was notably shown in photographs which they took of the spectrum of zinc. Unsymmetrical division of the lines by reversal would also come in, if the cooler and hotter portions of the gas were possessed of relative motion in the line of sight.

* [M. Deslandres permits me to quote the results of his recent observations on this point:—"Lorsque l'on dirige sur la fente d'un spectroscope de grande dispersion l'image d'une facule du soleil on a invariablement avec les raies H et K du calcium un renversement triple. Même lorsque les facules sont larges et intenses, on obtient encore le renversement triple avec des raies brillantes centrales, plus faibles il est vrai, si l'on envoie dans le spectroscope la lumière de tous les points du soleil, comme c'est le cas pour les étoiles; par exemple en dirigeant le collimateur vers le soleil sans l'intermédiaire d'aucun objectif, ou encore en le dirigeant vers un point quelconque du ciel. Si les facules sont au centre la raie centrale est à sa place normale, si elles sont à l'est ou à l'ouest la raie centrale est déplacée légèrement (2 kil. au plus) mais déplacée sûrement. Au point de vue pratique cette propriété fournit un moyen de reconnaître l'état général de la surface solaire lorsque le soleil est caché par les nuages."—June 20.]

These observers met also with double reversals, which gave a triple character to the expanded single line. In one experiment, when sodium carbonate was introduced into the arc the reversed D lines were seen as a broad dark band with a bright diffuse band in the middle. As the sodium evaporated the band narrowed, and the bright line in the middle showed a second reversal within it. This was a case of threefold reversal.

There would seem to be little doubt but that the more or less divided character—sometimes unsymmetrically—of the bright and dark lines of the Nova, which appeared to be undergoing continual alterations, was due to the incoming upon the broader lines of narrow reversed lines, bright or dark, as the case might be. Provision must therefore be made for conditions favourable for such reversals in any hypothesis which is suggested to account for the phenomena of the new star.

Waning of the Star.—The first record of this star was its appearance as a star of the 5th magnitude on a plate taken at Cambridge, U.S., on December 10, 1891. No star so bright as the 9th magnitude was found at its place on a plate taken by Dr. Max Wolf on December 8. Combining the photographic magnitudes obtained at Greenwich with the visual ones made at the University Observatory, Oxford, and by Mr. Stone and Mr. Knott, we find that throughout February and the first few days of March the light of the star declined very slowly, but with continual and considerable fluctuations, from about the 4·5th magnitude down to the 6th magnitude. After March 7, the remarkable swayings to and fro of the intensity of the light, set up probably by commotions attendant on the cause of its outburst, calmed down, and the star fell rapidly and with regularity to about the 11th magnitude by March 24, and then down to about 14·4th magnitude by April 1. On April 26, however, it was still visible at Harvard Observatory, magnitude 14·5, on the scale of the meridian photometer.

We observed its spectrum for the last time on March 24, when it had fallen to nearly the 11th magnitude. We were still able to glimpse the chief features of its spectrum. The four bright lines in the green were distinctly seen, and appeared to retain their relative brightness; F the brightest, then the line near *b*, followed by the lines about λ 5015 and λ 4921.

Traces of the continuous spectrum were still to be seen. Considering the much greater faintness of the continuous spectrum when the star was bright on February 2 than the brilliant lines falling upon it, we are not prepared to say that the falling off of the continuous spectrum was greater than might well be due to the waning of the star's light.

Professor Pickering informs me that on his plates "the principal bright lines faded in the order K, H, *a*, F, *h*, and G, the latter line

becoming much the brightest when the star was faint." The calcium lines K and H showed signs of variation during the whole time of the star's visibility, and I may remark that the order of the other lines agrees with the relative sensitiveness of the gelatine plate for these parts of the spectrum. Professor Pickering's photographic results appear to us to be in accordance with those we arrived at by eye, in not showing any material alteration in the nature of the star's light, notwithstanding its very large fall of intensity.

General Conclusions.

Among the principal conditions which must be met by any theory put forward to account for the remarkable phenomena of the new star stands the persistence without any great alteration—though probably with small changes—of the large relative velocity of about 550 miles a second in the line of sight between the hydrogen which emitted the bright lines and the cooler hydrogen producing the dark lines of absorption.

If we assume two gaseous bodies, or bodies with gaseous atmospheres, moving away from each other after a near approach, in parabolic or hyperbolic orbits, with our Sun nearly in the axis of the orbits, the components of the motions of the two bodies in the line of sight, after they had swung round, might well be as rapid as those observed in the new star, and might continue for as long a time without any great change of relative velocity. Unfortunately information as to the motions of the bodies at the critical time is wanting, for the event through which the star became suddenly bright had been over for some forty days before any observations were made with the spectroscope.

Analogy from the variable stars of long period would suggest the view that the near approach of the two bodies may have been of the nature of a periodical disturbance, arising at long intervals in a complex system of bodies. Chandler has recently shown in the case of Algol that the minor irregularities in the variation of its light are probably caused by the presence of one or more bodies in the system, besides the bright star and the dusky one which partially eclipses it. To a similar cause are probably due the minor irregularities which form so prominent a feature in the waxing and waning of the variable stars as a class. We know, too, that the stellar orbits are usually very eccentric. In the case of γ Virginis the eccentricity is as great as 0·9, and Auwers has recently found the very considerable eccentricity of 0·63 for Sirius.

The great relative velocity of the component stars of the Nova, however, seems to point rather to the casual near approach of bodies possessing previously considerable motion; unless we are willing to

concede to them a mass very great as compared with that of the Sun. Such a near approach of two bodies of great size is very greatly less improbable than would be their actual collision. The phenomena of the new star, indeed, scarcely permit us to suppose even a partial collision; though if the bodies were very diffuse, or the approach close enough, there may have been possibly some mutual interpenetration and mingling of the rarer gases near their boundaries.

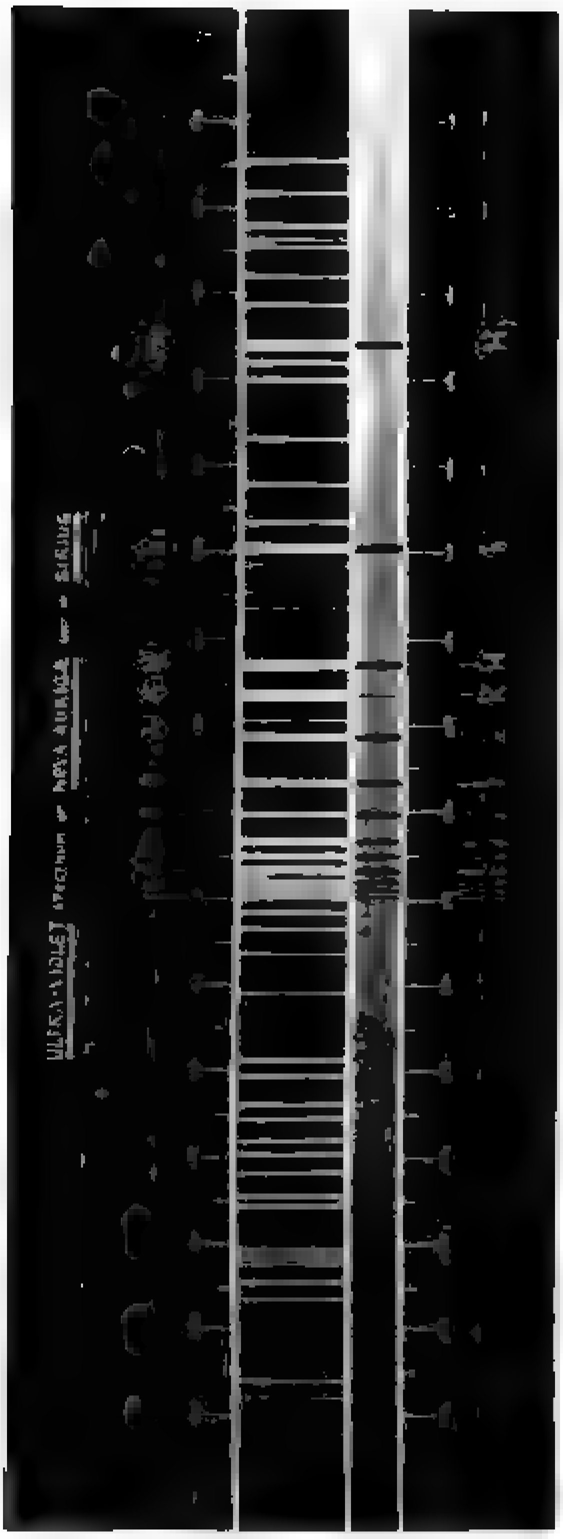
A more reasonable explanation of the phenomena, however, may be found in a view put forward many years ago by Klinkerfues, and recently developed by Wilsing, that under such circumstances of near approach enormous disturbances of a tidal nature would be set up, amounting it may well be to partial deformation in the case of gaseous bodies, and producing sufficiently great changes of pressure in the interior of the bodies to give rise to enormous eruptions of the hotter matter from within, immensely greater, but similar in kind, to solar eruptions; and accompanied probably by large electrical disturbances.

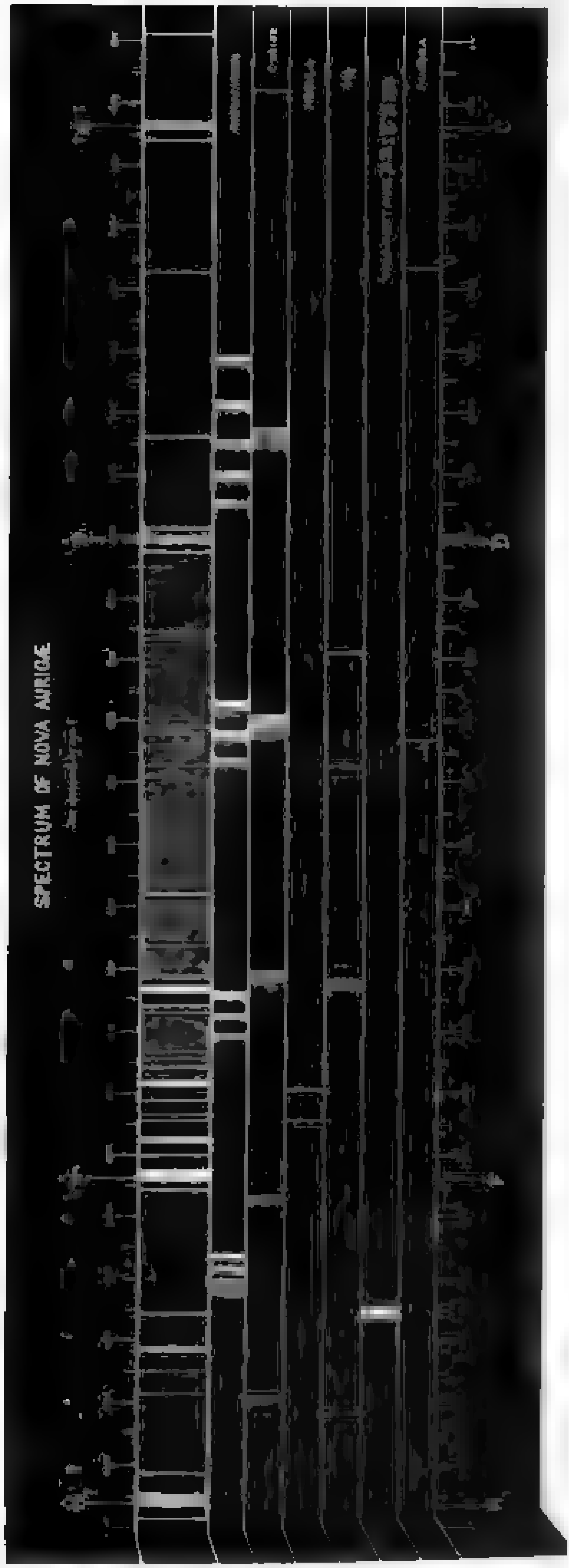
In such a state of things we should have conditions so favourable for the production of reversals undergoing continual change, similar to those exhibited by the bright and dark lines of the Nova, that we could not suppose them to be absent; while the integration of the light from all parts of the disturbed surfaces of the bodies would give breadth to the lines, and might account for the varying inequalities of brightness at the two sides of the lines.

The source of the light of the continuous spectrum, upon which were seen the dark lines of absorption shifted towards the blue, must have remained behind the cooler absorbing gas; indeed, must have formed with it the body which was approaching us, unless we assume that both bodies were moving exactly in the line of sight, or that the absorbing gases were of enormous extent.

The circumstance that the receding body emitted bright lines, while the one approaching us gave a continuous spectrum with broad absorption lines similar to a white star, may, perhaps, be accounted for by the two bodies being in different evolutionary stages, and consequently differing in diffuseness and in temperature. Indeed in the variable star β Lyræ, we have probably a binary system, of which one component gives bright lines, and the other dark lines of absorption. We must, however, assume a similar chemical nature for both bodies, and that they existed under conditions sufficiently similar for equivalent dark and bright lines to appear in their respective spectra.

We have no knowledge of the distance of the Nova, but the assumption is not an improbable one that its distance was of the same order of greatness as that of the Nova of 1876, for which Sir Robert Ball failed to detect any parallax. In this case, the light-





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emission suddenly set up, certainly within two days and possibly within a few hours, was probably much greater than that of our Sun; yet within some fifty days after it had been discovered, at the end of January, its light fell to about 1/300th part, and in some three months to nearly the 1/10,000th part. As long as its spectrum could be observed the chief lines remained without material alteration of relative brightness. Under what conditions could we suppose the Sun to cool down sufficiently for its light to decrease to a similar extent in so short a time, and without the incoming of material changes in its spectrum? It is scarcely conceivable that we can have to do with the conversion of gravitational energy into light and heat. On the theory we have ventured to suggest, the rapid calming down, after some swayings to and fro of the tidal disturbances, and the closing in again of the outer and cooler gases, together with the want of transparency which might come in under such circumstances, as the bodies separated, might account reasonably for the very rapid and at first curiously fluctuating waning of the Nova; and also for the observed absence of change in its spectrum.

I may, perhaps, be permitted to remark that the view suggested by Dr. William Allen Miller and myself, in the case of the Nova of 1866,* was essentially similar, in so far as we ascribed it to erupted gases. The great suddenness of the outburst of that star, within a few hours probably, and the rapid waning from the 3·6 magnitude to the 8·1 magnitude in nine days, induced us to throw out the additional suggestion that possibly chemical actions between the erupted gases and the outer atmosphere of the star may have contributed to its sudden and transient splendour; a view which, though not impossible, I should not now, with our present knowledge of the light-changes of stars, be disposed to suggest.

II. "On the Changes produced by Magnetisation in the Length of Iron and other Wires carrying Currents." By SHELFORD BIDWELL, M.A., LL.B., F.R.S. Received April 28, 1892.

The changes of length attending the magnetisation of rods or wires of iron and other magnetic metals which were first noticed by Joule† in 1841, and have in recent years formed the subject of many experiments by myself,‡ have been found to be related to several other phenomena of magnetism. Maxwell§ has suggested

* 'Roy. Soc. Proc.,' vol. 15, p. 146.

† 'Joule's Scientific Papers (Phys. Soc.),' pp. 48, 235.

‡ 'Phil. Trans.,' vol. 179, A, p. 205; 'Roy. Soc. Proc.,' No. 237 (1885), p. 265; No. 242 (1886), p. 109; No. 243 (1886), p. 257; vol. 43, p. 406; vol. 47, p. 469.

§ 'Electricity and Magnetism,' vol. 2, § 448.

that they sufficiently account for the twist which is produced in an iron wire when magnetised circularly and longitudinally at the same time. The resultant lines of magnetisation, as he points out, take a spiral form; the iron expands in the direction of the lines of magnetisation, and thus the wire becomes twisted. Professor G. Wiedemann, however, to whom the discovery of the magnetic twist is due, appears not to be satisfied with this explanation,* believing the effect to be caused by unequal molecular friction.

The subject of magnetic twists has been very fully and carefully investigated by Professor C. G. Knott, and in a paper published last year in the 'Transactions of the Royal Society of Edinburgh' (vol. 36, Part II, p. 485) he indicates many details in which the phenomena of twist closely correspond with those of elongation and retraction. Assuming their essential identity, and noting that "an increased current along the wire affects the points of vanishing twist in a manner opposite to that in which an increased tension affects it," Professor Knott is "inclined to conclude that the pure strain effects of these influences are of an opposite character." Now, since the magnetic elongation of an iron wire is known to be diminished by tension, the remark above quoted amounts to a prediction that in an iron wire carrying a current the magnetic elongation would be increased. "We know nothing so far," Professor Knott observes, "regarding the changes of length when an iron wire *carrying a current* is subjected to longitudinal magnetising forces," and it was with the object of acquiring some information on this point and testing Professor Knott's prediction that the experiments described in the present paper were undertaken. The results show that it was amply verified, and thus Maxwell's explanation of the twist receives still further corroboration.

The apparatus used and the methods of observation were the same as those described in my former papers. Each specimen of wire examined was 10 cm. long between the supporting clamps, and the magnetising coil, weighing nearly 3 lbs., was as usual supported by the wire itself, an arrangement which, for reasons before given, was essential. The indications of the instrument were read to one ten-millionth part of the length of the wire, and the wire was demagnetised by reversals before each single observation.

Exp. 1.—The wire first used was of soft commercial annealed iron, 0·75 mm. in diameter. The changes of length which it exhibited under the influence of magnetising forces gradually increased from 13 to 315 C.G.S. units, are indicated in the second column of Table I, in which the unit is one-millionth of a centimetre or one ten-millionth of the effective length of the wire. The magnetising forces given in the first column are those due to the coil only, no account being taken

* 'Phil. Mag.,' July, 1886, p. 50.

of the demagnetising effect of the wire. The results are also plotted as a curve in fig. 1. It will be seen that the maximum increment of length, attained in a field of about 40, was 11·5 ten-millionths; the decrement of length in a field of 315 was 22·5, while the original length of the wire was unchanged in a field of 130.

Table I.—Iron Wire, diameter 0·75 mm.

Magnetic field due to coil. C.G.S. units.	Elongations in ten-millionths of lengths.		
	With no current through wire.	With 1 ampère through wire.	With 2 ampères through wire.
13	3	7	
16	6	9	11·5
23	7·5	12	
34	10	14·5	20
40	11·5	14	
50	10	14	20
61	9·5	12	
81	6	9·5	16
97	4	8	
130	0	3·5	8
171	−4	0	
202	−9	−4	−1
244	−13·5		
250	..	−9	−5
315	−22·5		
319	..	−18·5	
323	..		−13

Exp. 2.—A current of 1 ampère was then passed through the wire. The current, which was derived from a Grove's cell, was measured by a tangent galvanometer and regulated by a rheostat, which had been approximately adjusted on the previous day. As soon as the circuit was closed, the index of the measuring instrument began to move, rapidly at first and afterwards more slowly, in the direction indicating elongation of the iron wire. In about two minutes the index had come to rest again, the number of scale divisions over which it had passed showing that the original length of the wire had increased by 310 ten-millionths. Assuming the coefficient of expansion of the iron to be 122 ten-millionths per degree Centigrade this elongation denoted a rise of temperature (due to current heating) of about 2°·5. The experiment described in the last paragraph was then repeated, the several magnetising forces employed being made as nearly as possible the same as before by inserting the same resistances successively in the circuit.* The results appear in the third

* Independent readings of the ampère meter were taken in the two experiments, and the readings corresponding to the same resistance in both series all agreed

column of Table I and in the middle curve of fig. 1. The latter shows clearly that the maximum elongation had risen from 11·5 to 14·5 ten-millionths, while the decrement in a field of 315 had fallen from 22·5 to about 17·5.

Exp. 3.—The current through the iron wire was then increased, by an alteration of the rheostat, to 2 ampères. The further elongation of the wire due to the heating effect of the increased current was very nearly 1000 ten-millionths, corresponding to a rise of temperature of 8°·2 C. This added to 2·5, the rise due to the current of 1 ampère, which was passing before, gives 10°·7 as the excess of the temperature of the wire carrying 2 ampères above that of the room. When the index had become steady, which happened in the course of about $2\frac{1}{2}$ minutes, another series of observations was made; but instead of applying all the previously employed magnetising forces in succession, alternate ones were omitted. This was done for the purpose of shortening the experiment, it being thought doubtful whether the Grove's cell which supplied current to the iron wire would remain sufficiently constant when giving so strong a current as 2 ampères. The results of the experiment are contained in the last column of Table I, and in the highest of the curves in fig. 1. There is again a marked increase of the maximum elongation, and decrease of the retraction in a field of 315.

For the sake of easy comparison, the principal results obtained with this wire are collected in Table II.

Table II.—Iron Wire, diameter 0·75 mm.

Current through iron wire. Ampères.	Maximum elongation in ten-millionths of length.	Retraction in field of 315 C.G.S. units.	Field in which length is unchanged.
0	11·5	22·5	130
1	14·5	17·5	170
2	20	12	200

Exp. 4.—The previous observations were repeated with another specimen of soft iron wire of greater diameter, viz.: 1·05 mm., no current being at first passed through it. The results appear in the second column of Table III and in fig. 2.

within a quarter of a scale division, with the exception of the two last, which showed that the E.M.F. of the battery—seven Grove's cells—was slightly increasing, or rather perhaps that its internal resistance was diminishing. When two successive readings with the same resistance in circuit differed by no more than a quarter of a scale division (equivalent to 3·125 units of magnetising force), the mean of the two readings was taken as giving the true current.

Table III.—Iron Wire, diameter 1·05 mm.

Magnetic field due to coil. C.G.S. units.	Elongation in ten-millionths of length.	
	With no current through wire.	With 2 ampères through wire.
7	1	2·5
16	6·5	11
25	..	15
34	13	18
40	14	18
50	12·5	18·5
62	12	18
87	10	16
134	3·5	8
213	−5·5	−1
263	−10·5	−8
338	−20	−16·5

Exp. 5.—A current of 2 ampères was passed through the same wire, resulting in an elongation due to heating of 460 ten-millionths, the temperature of the wire being therefore raised about 3°·3. The former observations were again made with the results given in the last column of Table III and in fig. 2.

It will be seen that with both specimens of iron wire, the effect of a current is of just the same general character. It acts oppositely to tension, heightening the curve of elongation instead of lowering it. This action is certainly not due either directly or indirectly to mere current heating. It has been shown that the thinner wire even when carrying 2 ampères was only about 10°·7 warmer than when no current was passing through it. Such a small rise of temperature would be quite incompetent by itself to account for the effect in question, for the elongation curves of a given specimen of iron have been found to be not sensibly altered when taken under widely different conditions of temperature. Nor would it exert any material influence upon the susceptibility of the iron; and, even if it did, the curves would not be affected in the manner observed.

It is hardly worth while attempting to frame an explanation until many more phenomena of the same order have been investigated.

Similar experiments were afterwards made with nickel and cobalt.

Exp. 6.—A nickel wire was used, the diameter of which was 0·65 mm. The retractions which it underwent in fields of gradually increasing strength are given in the second column of Table IV.

Table IV.—Nickel Wire, diameter 0·65 mm.

Magnetic field due to coil. C.G.S. units.	Retractions in ten-millionths of length.		
	With no current through wire.	With 1 ampère through wire.	Difference.
12	8	8	0
15	10	11	−1
19	15	15	0
28	25·5	25	0·5
36	34	33	1
50	50	48	2
69	74	72	2
84	92	92	0
99	113	112	1
119	134	133	1
150	164	162	2
175	178	178	0
209	196	194	2
256	217	215	2
330	241	240	1

Exp. 7.—A current of 1 ampère was passed through the nickel wire, producing a heat elongation of 340 ten-millionths. Taking the coefficient of expansion as 0·0000129, this implies a rise of temperature of 2°·6. The retractions of the wire when carrying a current are given in the third column of the table. Remembering that the figures in the second and third columns denote millionths of a centimetre, the close agreement between the two is very remarkable. I have elsewhere* fully described the method of observation adopted, but I may perhaps mention that each number as set down in the table was obtained by the subtraction of two readings, the one taken when there was no current in the magnetising coil, the other when the current was turned on. The former or zero reading was continually changing, owing to small alterations of temperature, the index rarely being absolutely at rest. All the figures were dictated, and when the second experiment was made, I had not seen the results of the first. I may add that the table contains *all* the observations which were taken in the two experiments.

Though at first inclined to attribute such small discrepancies as exist entirely to observational or instrumental errors and to infer that the current had no influence whatever upon the contraction, I think it appears pretty clearly from a careful inspection of the differences tabulated in the fourth column that this is not actually the case. Four pairs of observations agree exactly; once only the retraction with the

* 'Phil. Trans.,' vol. 179, A, p. 218.

current seems to be greater than without it, while in the ten remaining pairs the retraction is slightly greater without the current than with it. It may, perhaps, be fairly concluded that the current has a real but very small effect in diminishing the retraction. Now I have before remarked that the degree of retraction which nickel undergoes when magnetised is materially affected by comparatively small changes of temperature: the retraction of the same specimen has been found to be greater in a cold room than in a warm one, at least in fields up to 400 or 500. Probably this is to be explained by the influence of heat in diminishing the magnetic susceptibility of nickel, the retractions being really the same for the same intensity of magnetisation. Such small effect as appears to be produced by the action of the current may, therefore, be accounted for simply by the rise of temperature (2°·6) which it causes.

Tension has a large effect upon the magnetic retraction of nickel:* it is, therefore, the more remarkable that the action of a current, which operates so markedly upon iron, should in nickel be practically insensible.

Exp. 8.—The results with no current obtained for a strip of rolled cobalt, the length of which between the clamps was 10 cm., and the cross section 1·82 sq. mm., are given in the first two columns of Table V.

Table V.—Cobalt Strip, section 1·82 sq. mm.

Magnetic field due to coil. C.G.S. units.	Retraction in ten-millionths of length.		
	With no current through strip.	With 2 ampères through strip.	Difference.
34	1	1	0
50	2	2·5	−0·5
84	4	5	−1
100	6	6	0
119	7·5	8·5	−1
153	11	11·5	−0·5
209	16	16·5	−0·5
331	26	27·5	−1·5

Exp. 9.—A current of 2 ampères through the strip caused a heat elongation of about 600 ten-millionths, indicating, if the coefficient of expansion is taken as 0·0000125, a rise of temperature of 4°·8. The retractions observed while this current was passing are set out in the third column of the table. From an inspection of the differences

* 'Roy. Soc. Proc.,' vol. 47, p. 469.

tabulated in the fourth column, it appears that the effect of the current is to increase the retraction very slightly.

According to Rowland the susceptibility of cobalt is *increased* by heating. The small additional retraction indicated when the current was passing was, therefore, no doubt due to the increased susceptibility consequent upon current heating. It may be noted that tension seems to have no material effect upon the magnetic retraction of cobalt.*

Summary.

In an iron wire carrying a current, the maximum magnetic elongation is greater, and the retraction in strong fields is less, than when no current is passing. The effect of the current is opposite to that of tension.

The magnetic retractions of nickel and of cobalt are not sensibly affected by the passage of a current through the metals. (Tension considerably modifies the magnetic retraction of nickel, but not that of cobalt.)

III. "On the Measurement of the Magnetic Properties of Iron." By THOMAS GRAY, B.Sc., F.R.S.E. Communicated by LORD KELVIN, P.R.S. Received May 3, 1892.

(Abstract.)

This paper gives the method of experiment and results obtained in some investigations on the time-rate of rise of current in a circuit having large electromagnetic inertia. The experiments were made on a circuit containing the coils of a large electromagnet having laminated cores and pole pieces. The mean length of the iron circuit was about 250 cm. and its cross section 320 sq. cm. The magnetising coil had 3840 turns, when all joined in series, and a resistance of 10·4 ohms. The coils were so arranged that they could be joined in a variety of ways so as to vary the resistance, inductive coefficient, &c., and also to allow the magnet to be used either as an open or a closed circuit transformer.

The electromotive force used in the experiments was obtained from a storage battery, and the method of experiment was to trace the curve, giving the relation of current to time, on a chronograph sheet.

One set of experiments shows the effect of varying the impressed E.M.F. on the time required for the current to attain any given percentage of its maximum strength. The results show that for any particular percentage there is always a particular E.M.F. which takes

* *Loc. cit.*

maximum time. Thus for the circuit under consideration, and with successive repetitions of the current in the same direction, it takes longer time for the current produced by an impressed E.M.F. of 4 volts to reach 95 per cent. of its maximum than it takes for the current produced by either 3 or 5 volts to reach 95 per cent. of their maximum. The results show also that, within considerable limits, the time required for the current to become uniform is on the whole nearly inversely proportional to the impressed E.M.F., and that for moderate values of the E.M.F. the time may be very great; when the E.M.F. was 2 volts, and the current sent in such a direction as to reverse the magnetism left in the magnet by a previous current of the same strength, the time required for the current to establish itself was over three minutes. The difference of time required for repetition and for reversal of previous magnetisation was also very marked when the iron circuit was closed. The results show that great errors may arise by the use of ballistic methods of experiment, especially when weak currents are used, and that for testing resistances of circuits containing electromagnets, a saving of time may be obtained by using a battery of considerable E.M.F.

Another set of experiments gives the effect of successive reversals of the impressed E.M.F. at sufficient intervals apart to allow the magnetisation to be established in each direction before reversal began. In this set also the effect of cutting out the battery and leaving the magnet circuit closed is illustrated, showing that several minutes may be required for the magnet to lose its magnetism by dissipation of energy in the magnetising coil. The effect on these cycles of leaving an air space in the iron circuit is also illustrated. It is shown that a comparatively small air space nearly eliminates the residual magnetism and diminishes considerably the rate of variation of the coefficient of induction and the dissipation of energy in the magnet.

Several cycles are shown for the magnet used as a transformer with different loads on the secondary. The results give evidence that there is less energy dissipated in the iron the greater the load on the secondary of the transformer.

Some experiments are also quoted which go to show that the dissipation of energy due to magnetic retentiveness (magnetic hysteresis) is simply proportional to the total induction produced when the measurements are made by kinetic methods. Reference is made to the recent experiments of Alexander Siemens and others which seem to confirm this view.

IV. "On the Development of the Stigmata in Ascidians." By WALTER GARSTANG, M.A., Jesus College, Oxford; Berkeley Fellow of the Owens College, Manchester. Communicated by A. MILNES MARSHALL, M.A., M.D., F.R.S. Received April 14, 1892.

The respiratory organ or pharynx of the Tunicata exhibits a great amount of variability in form, size, and complexity of structure in the different members of the group—a variability which is obviously correlated with the physiological value of the organ. In the simplest forms, the Perennichordata or Appendicularians, the pharynx is a mere hollow cylinder, provided with a single pair of tubular gill-clefts, one on each side. In the higher forms (the Caducichordata) the cavities of the two gill-clefts become enormously dilated, so as to constitute a pair of peribranchial chambers interposed on either side between the pharynx and the body-walls. The dilatation of the gill-clefts to form peribranchial chambers can be aptly compared with the formation of branchial pouches in the tubular gill-clefts of Marsipobranch Fishes. But whereas the respiratory surface of the branchial pouches of Marsipobranchs is increased by folds of the walls of the pouches, the same purpose in the higher Tunicata is effected by a different means. The inner (visceral) walls of the peribranchial chambers apply themselves closely to the wall of the pharynx, and perforations then appear at numerous points, where the pharyngeal and peribranchial membranes have actually united. The remnants of the pseudo-cœlomic cavity, enclosed between the pharynx and the visceral walls of the peribranchial chambers, become extensive channels for a vigorous circulation of the hæmal fluid. The organ formed by the union of the pharyngeal and peribranchial walls is usually referred to as the "branchial sac;" and the perforations, which put the cavity of the pharynx into extensive communication with the peribranchial chambers, constitute the so-called "stigmata."

The stigmata vary much in form and arrangement. In the fixed Ascidians, whether simple or compound, they are usually simple slits, of a narrow elongated form, arranged in a series of rows placed transversely to the longitudinal axis of the body (*Ascidia*, *Clavelina*, *Botryllus*, *Styela*). In some genera, however (*Corella*, *Molgula*), the stigmata are curved and somewhat spirally arranged; but this condition is undoubtedly derived from the former by modifications of a secondary nature. In the pelagic Tunicata (*Salpa*, *Doliolum*, *Anchinia*, *Pyrosoma*), the condition met with in the fixed Ascidians is never found; there is never more than one row of stigmata on each side, and this row, though occasionally oblique or even transverse, is

usually longitudinal in direction. In *Pyrosoma* and some species of *Doliolum* the stigmata are narrow, elongated slits, extending transversely across the whole lateral face of the pharynx—each stigma occupying an area which in the fixed Ascidians is taken up by an entire transverse row of stigmata.

To ascertain what is the fundamental order underlying all these variations, and to determine what degree of correspondence and homology there is between the stigmata of the pelagic Tunicata and those of the fixed Ascidians, is not an easy matter; indeed, the possibility of any detailed comparison hardly seems to have occurred to the majority of investigators. Two views, due to Herdman and Lahille respectively, are, however, worthy of mention here.

Professor Herdman* derives *Pyrosoma* from a group of the compound Ascidians, through the curious colonial form *Cœlocormus Huxleyi*; and, in harmony with this view, he regards each of the transverse stigmata of *Pyrosoma* as corresponding to an entire transverse row of stigmata in the Ascidians, the several stigmata of the row having apparently coalesced to form the single stigma of *Pyrosoma*—a process which has almost certainly occurred in certain deep-sea types (*Fungulus*, *Culeolus*, &c.).

Lahille† characterises the latter portion of Herdman's view as a "profound error," and attempts, instead, to establish the remarkable proposition that the longitudinal row of transverse stigmata in *Pyrosoma* is strictly homologous with one of the transverse rows of longitudinal stigmata in an Ascidian, through a phylogenetic rotation of position. The oblique position of the row of stigmata in some species of *Doliolum* (*e.g.*, *D. Ehrenbergi*) is regarded as an intermediate condition between the two extremes. Lahille bases this proposition upon the changes of position which the organs of a *Pyrosoma*-bud undergo during development. These changes, it is true, are very remarkable, but they furnish absolutely no evidence for Lahille's contention; for it is a well-established fact‡ that immediately after their first appearance the stigmata of *Pyrosoma* begin to elongate in a direction at right angles to the long axis of the endostyle, and this relation is maintained through all the curious changes of form which the bud undergoes in its further development. Lahille's homologies are consequently without foundation; and, although Herdman's comparison is far more justifiable, yet the development of the transverse

* '"Challenger" Reports,' "Tunicata," 2nd Report, pp. 319, 320; 3rd Report, pp. 20, 24, 25, 137.

† 'Recherches sur les Tuniciers,' Toulouse, 1890, pp. 59, 61, figs. 44—51.

‡ Seeliger: "Zur Entwickelungsgeschichte der Pyrosomen;" 'Jenaische Zeitschrift,' vol. 23, 1889, p. 622, figs. 15, 17, 19. Salensky: "Beitr. z. Entwick. d. Pyrosomen;" 'Zoolog. Jahrbüch., Abth. f. Anat.,' vol. 5, 1891, p. 32. Salensky's figures 2 and 3, on p. 9, are, however, strangely inaccurate in this respect.

stigmata in *Pyrosoma* clearly tends to show that these are simple structures, and that they have not arisen by the modification of so many transverse rows of stigmata, as his theory demands.

The question is thus seen to be still unsolved, and if in this communication I venture to offer some observations upon the matter, it is in the belief that they tend considerably to elucidate the problem.

The view that the pelagic Caducichordate Tunicata have been profoundly modified in structure through their mode of life, and that they are to be derived phylogenetically from the so-called Compound Ascidians, is at present held, with varying reservations, by almost every recent investigator of the Tunicata, except Seeliger. It is held by Grobben and Uljanin for *Pyrosoma*, *Salpa*, and *Doliolum*, by Herdman for *Pyrosoma*, by Lahille for *Pyrosoma* and *Doliolum*, and by Salensky for *Pyrosoma* and *Salpa*. The evidence for this view has always seemed to me to be very slender and unimportant; and I believe it is this widely-spread conception which is answerable, among other things, for the absence of any satisfactory comparison between the stigmata of the fixed Ascidians and of their pelagic allies.

I have accordingly approached the question from the reverse point of view, believing that, by a study of the development of the stigmata in the fixed Ascidians, recapitulative stages would be met with which would furnish the desired answer. A grant awarded me by the Government Grant Committee last year enabled me, during the summer, to collect suitable material at the Plymouth station, and my observations have been completed in Professor Milnes Marshall's laboratories at the Owens College.

The development of the stigmata in the larva and oozooid of Ascidians has hitherto been very little investigated. The earliest complete account is that of Krohn,* an abstract of whose observations upon *Phallusia mammillata* is given by Balfour ('Comp. Emb.,' vol. 2, p. 20). Krohn's interpretations were subsequently criticised by E. van Beneden and Julin in their valuable paper† on the "Post-embryonic Development of *Phallusia* (*Ascidiella?*) *scabroïdes*." These investigators showed that in the latter species two stigmata at first appear, one behind the other, on each side of the pharynx, and that subsequently new stigmata arise between and behind the two first, until a longitudinal row of six stigmata is formed on each side. These stigmata enlarge transversely to the long axis of the body, and subsequently subdivide, in the order of their formation, so as to constitute a corresponding number of transverse rows of smaller stigmata. Van Beneden and Julin thus drew a distinction between primary stigmata and secondary (definitive) stigmata, and called attention to the irregular order of formation of the primary stigmata as a point

* "Ueber die Entwicklung d. Ascidien," 'Müller's Archiv,' 1852.

† 'Arch. de Biologie,' vol. 5, 1884, p. 611.

worthy of notice. They did not, however, draw any general conclusions from the phenomena which they observed, beyond pointing out that the irregular order in which the primary stigmata appeared was in opposition to any theory as to their metameric arrangement.

The only other observations of importance are those of Seeliger* on the development of the stigmata in *Clavelina*. In this form, as in *Phallusia* and *Molgula*,† two pairs of stigmata at first arise, one behind the other, near the dorsal border of the sides of the pharynx. But instead of elongating in a transverse direction, as is the case in *Phallusia*, these stigmata elongate in a longitudinal direction, and become directly converted into the stigmata of the adult. With the downward extension of the peribranchial chambers, new stigmata arise independently, below the two first formed, so that eventually two transverse rows of perforations are formed on each side of the pharynx; and all these, by growth in a longitudinal direction, become directly converted into the slit-like stigmata of the adult. Subsequently, after the attachment of the larva, new transverse rows of stigmata arise in front of and behind the two first rows in an identical manner.

I have myself followed out the development of the stigmata in *Clavelina*, and have nothing to add to, or alter in, Seeliger's description; the stigmata invariably arise quite independently, and I have seen no indication of such a process of subdivision as has been described above for *Phallusia (Ascidiella ?) scabroïdes*.

A similar independent mode of origin of the stigmata has also been observed by Giard‡ in *Perophora*, and by Lahille§ in *Distaplia magnilarva*.

Thus, up to the present time, we are acquainted with three distinct genera in which the stigmata arise independently of one another; while the process of subdivision, described for *Phallusia (Ascidiella ?) scabroïdes*, remains unconfirmed and entirely without parallel. It would even be excusable to regard this latter method, from its exceptional character, as a developmental modification of the former. But before discussing this diversity of development, I will describe certain observations which I have made as to the development of the stigmata in several other types of Ascidians.

In *Botryllus* the stigmata of the adult have the usual form of

* "Zur Entwicklungsgesch. d. Socialen Ascidien," 'Jen. Zeit.,' vol. 18, 1885, pp. 45—150, Plates 1 to 8.

† P. J. van Beneden (*M. ampulloïdes*). Kupffer, 'Arch. f. Mikr. Anat.,' vol. 8, 1872, Taf. 17, fig. 8*a*. Lacaze-Duthiers, 'Arch. de Zool. Exp.,' vol. 3, 1874, pp. 623, 631, Plate 27.

‡ 'Arch. de Zool. Exp.,' vol. 1, 1872, p. 677, Plate 24, fig. 6.

§ 'Recherches sur les Tuniciers,' p. 165.

longitudinally elongated slits, arranged in a series of transverse rows on each side of the pharynx.

Various points in the development of *Botryllus* have been elucidated by the researches of Metschnikoff,* Krohn,† and Ganin,‡ but the development of the stigmata remains still undescribed.

The stigmata of the oozooid arise in a manner very different from that which we have seen in the case of the compound Ascidians *Clavelina*, *Perophora*, and *Distaplia*; the mode of their development recalls the phenomena described by E. van Beneden and Julin for "*Phallusia*" *scabroïdes*, but presents distinctive features of considerable importance.

In the earliest stage which has come under my observation, the young zooid (*B. aurolineatus*, Giard) is already fixed and is provided with the rudiments of two buds, one on each side. The endodermic vesicles of the buds as yet show no signs of differentiation. The zooid itself possesses the rudiments of two lateral tentacles only, and is provided with the eight club-shaped ectodermic processes, with long stalks, which are so characteristic of the larva.

The pharynx is provided with four pairs of transversely elongated stigmata, whose transverse diameters are nine times as great as their antero-posterior diameters. These huge transverse slits, whose width almost equals the length of the endostyle, extend right across the sides of the pharynx, from the dorsal region to the endostyle. They are not exactly of equal size, but decrease slightly in width in regular order from before backwards. The second slit is 0·2 mm. wide. The endostyle at this stage is 0·25 mm. long.

In the next stage examined (*B. aurolineatus*) the endodermic vesicle of each bud is already differentiated into a median pharyngeal portion and a pair of lateral peribranchial portions. The oozooid has now the rudiments of four tentacles, and the number of ectodermic processes has increased to eleven, the separate stalks being now very short.

The pharynx possesses, in place of the four pairs of transversely elongated stigmata, four transverse rows of small stigmata on each side. The anterior row is 0·42 mm. wide, the second row is 0·35 mm. wide, and the two posterior rows are still narrower. The endostyle at this stage is 0·425 mm. long. The first row consists of 10 stigmata, the second of 8 stigmata, the third of 6 or 7, the fourth of still fewer. None of the stigmata are elongated transversely; they are, for the most part, of an oval form, slightly elongated longitudinally, but towards the dorsal side they are more or less circular.

At a still more advanced stage (*sp. incert.*) the oozooid has attained

* 'Bull. Acad. Imp. Sci. St. Pétersbourg,' 1869, pp. 291—293.

† 'Arch. f. Naturgesch.,' vol. 35, 1869, pp. 190—196, 326—333.

‡ "Neue Thatsachen;" 'Zeit. f. Wiss. Zool.,' vol. 20, 1870.

very large dimensions, the number of ectodermic processes has increased to 36, and each of the lateral buds has given origin to a pair of buds of the second generation. At this stage there are five rows of stigmata on each side; but whether the fifth row is formed by the subdivision of a transverse primary stigma, or not, I have been unable to determine.

This is the most advanced stage in the progressive development of the oozooid of *Botryllus* that I have seen, and there is reason to believe that the number of rows of stigmata does not increase after this point. In a young colony, very little older than the one just described, the oozooid has undergone great reduction in size, and is evidently dying away, while the two buds of the first generation, to which it gave rise, have grown considerably in size, and are almost fully organised.

It is perfectly clear from the above account that in *Botryllus* the stigmata of the full-grown oozooid are secondary formations, due to the subdivision of a series of transversely elongated primary stigmata with which the larva is provided. It is also noteworthy that the primary stigmata—if an inference may be drawn from their relative sizes—arise one after another in regular order from before backwards, and that they are subsequently subdivided in the same order.

It will be convenient to distinguish the transversely elongated primary stigmata by some distinctive name, and on this account I propose for them the term "protostigmata." It cannot be denied that these structures present striking analogies with the true gill-clefts of *Amphioxus* and the lower Vertebrata.

Turning now to the buds of *Botryllus*, a remarkable difference is to be observed in the mode of origin of the stigmata, a difference which has important bearings upon the question of their phylogenetic history. Transverse protostigmata are never formed in the buds, whatever be the number of the generation to which the buds belong. The stigmata of the four first rows arise almost simultaneously as small rounded perforations which are entirely independent of one another; they soon begin to elongate in an antero-posterior direction and rapidly assume their definitive form.

Botryllus, therefore, exhibits both the modes of development which are known to occur in Ascidians; the stigmata in the oozooid arise by the subdivision of protostigmata, and the stigmata of the buds appear quite independently of one another. This fact renders it possible to determine which method is the more primitive. In any contrast of this sort between larval and bud development, there can be no doubt that it is the larva which exhibits the primitive mode, while the development in the bud is secondary and modified.

Now the development of the stigmata in the oozooids of *Clavelina* and *Distaplia* proceeds in essentially the same manner as in the buds

of *Botryllus;* and there can be, accordingly, just as little doubt that the development presented by the oozooids of these forms is also secondarily abbreviated. This conclusion is corroborated by the facts that in these genera and their allies the ova contain more food-yolk than is usual in Ascidians, and that the duration of the free-swimming larval stage is greatly reduced.

It is therefore obvious that the primitive (phylogenetic) mode of development of the stigmata in Ascidians is by the subdivision of transverse protostigmata arranged in a single longitudinal series on each side of the pharynx.

I have observed this process in two other species of Ascidians, *Thylacium sylvani* (Carus)* and *Styela (Styelopsis) grossularia* (van Beneden). These two species are very closely allied, and exhibit no appreciable differences in their mode of development.

In *Thylacium sylvani* eight protostigmata arise on each side of the pharynx, and become subdivided, in regular order from before backwards, to form a corresponding number of rows of secondary stigmata. The protostigmata extend right across the sides of the pharynx, as in *Botryllus*, before they are subdivided; and, although I have not actually observed their earliest stages, they give every appearance of having been formed in regular order from before backwards. The subdivision of the protostigmata begins towards their dorsal extremities and then extends ventrally—a process which compares very well with the formation of the stigmata in *Clavelina.* In the pharynx, at an early post-larval stage, and after the subdivision of the protostigmata has commenced, the actual nature of the process can easily be observed. Small projections arise from the anterior margins of the protostigmata, and are met by corresponding outgrowths from their posterior margins; the tips of these projections then coalesce with one another, and by their union give rise to the so-called interstigmatic bars of the fully constituted branchial sac. The shape of the secondary stigmata during the process of subdivision is often very irregular, but an admirable symmetry and regularity of form and arrangement is presented as soon as the subdivision is completed.

The following numbers illustrate the regular order in which the protostigmata are subdivided. They represent the numbers of secondary stigmata into which the protostigmata of one side have become converted in a pharynx 1·125 mm. long and 2·5 mm. in circumference, at a period when the stigmata of the first row have already assumed the form of narrow, longitudinally elongated slits :—

* I owe to Professor Ray Lankester the opportunity of examining the type-specimen of this species, which is in the collection of the Oxford University Museum. It was very erroneously described by its discoverer, and a redescription of it, which I have prepared, will be published shortly.

Row.	Number of stigmata.	Observations.
1	17	Length (antero-posterior) of stigmata, in middle of row, 0·25 mm.
2	16	Ditto, ditto, ditto, 0·125 mm.
3	10	Four are transversely elongated, one is round, and five are of a longitudinally oval form. Length, 0·075 mm.
4	3	One very wide stigma, and two longitudinally oval ones at the dorsal extremity.
5	2	One extremely wide stigma, and one small transversely elongated stigma at the dorsal extremity.
6 7 8	The three posterior protostigmata are completely undivided. The width of the 6th is 0·55 mm.; of the 7th, 0·425 mm.; and of the 8th 0·25 mm.	

The regular development of the protostigmata in *Botryllus* and *Thylacium* contrasts markedly with the phenomena observed in "*Phallusia*" *scabroïdes* by van Beneden and Julin, but it is very probable that the irregularity of formation in that species is the result of secondary changes. It may, I think, be safely concluded that the protostigmata of Ascidians arose primitively in regular order from before backwards.

It is very significant that in the pelagic Tunicate *Pyrosoma* the phylogenetic inferences which have here been drawn from the development of the stigmata in Ascidians are exactly fulfilled. In this form—as I have stated in the introduction—the stigmata are arranged in a single longitudinal series along each side of the pharynx, and they are transversely elongated, from the dorsal surface to the endostyle. They therefore resemble precisely, both in form and in arrangement, the protostigmata of larval Ascidians. Moreover, it appears from the recent researches of Salensky* that the stigmata in the ascidiozooids of *Pyrosoma* arise in regular order from before backwards, just as do the protostigmata in *Botryllus* and *Thylacium*. These resemblances are of too important a character to be mere coincidences. I would therefore submit that in *Pyrosoma* we have a primitive type of Caducichordate Tunicata, which is antecedent to the whole of the phylum Ascidiacea, and which exhibits very closely the ancestral form of pharynx from which the complicated respiratory organ of the fixed Ascidians has been derived.

It would further follow that *Clavelina* and its allies can no longer

* *Loc. cit.*, pp. 5, 33.

be regarded as the most primitive members of the order Ascidiacea, and that *Botryllus* and the Styelinæ must take this position; for in the structure and development of the pharynx, as well as in other points, with which I shall fully deal elsewhere, the latter forms approach, more nearly than any other Ascidians, the ancestral type represented by *Pyrosoma.*

V. "Observations on the Post-Embryonic Development of *Ciona intestinalis* and *Clavelina lepadiformis.*" By ARTHUR WILLEY, B.Sc. Lond. Communicated by Professor RAY LANKESTER, M.A., F.R.S. Received May 4, 1892.

The following is an account of some of the observations which were made by the author during an occupation of the British Association Table at the Zoological Station at Naples from October, 1891, to May, 1892.

In their admirable "Recherches sur la Morphologie des Tuniciers," ('Archives de Biologie,' vol. 6, 1887), Édouard van Beneden and Charles Julin came to a number of conclusions which, while they appeared to follow naturally from the facts observed, yet only added, if possible, to the perplexity surrounding any attempt to regard the Ascidians and *Amphioxus* from a common standpoint. Led away by the remarkable behaviour of the endostyle which I observed and described in the larva of *Amphioxus*, I easily induced myself to accept the views of the Belgian *savants.*

The observations on the post-embryonic development of *Ciona* described below oblige me, however, to reconsider the position which I took in my paper on "The Later Larval Development of *Amphioxus*" ('Quart. Journ. Micro. Sci.,' vol. 32, 1891), with regard to the mutual relations of the Ascidians and *Amphioxus*, and may, I hope, tend to the establishment of reasonable homologies between them.

It is necessary to recapitulate very briefly the views of van Beneden and Julin, in order to bring those which I am about to oppose to them in the most striking contrast.

The following table shows at a glance the homologies suggested by the above-named authors:—

(*a.*) The anterior intestinal diverticula of *Amphioxus*, the right one of which becomes the large head- or, better, proboscis-cavity, while the left becomes the præoral pit.	=	The primary branchial canals of Ascidians (*i.e.*, the first pair of gill-slits; see below).

(*b.*) The club-shaped gland of *Amphioxus.* } = The intestine of Ascidians.

(*c.*) Gill-slits of *Amphioxus, unrepresented* in Ascidians.

(*d.*) Atrial cavity of *Amphioxus, unrepresented* in Ascidians.

Of the above propositions, the last may be true; but I shall proceed to show that the others are untenable.

Fixation of the Larva of Ciona.

Just before the larva fixes itself, a narrow space can be discerned between the anterior end of the endoderm and the tract of ectoderm which bears the adhering papillæ; and in it lies a compact group of mesoderm cells.

Shortly after fixation this space swells up prodigiously, and then contains loose scattered mesoderm cells. I shall call this the *proboscis-cavity.* At its base (*i.e.*, where it joins on to the body) lies the endostyle dorso-ventrally. The primary position of the endostyle is extremely important. It behaves exactly as it does in the larva of *Amphioxus*, in that it occupies at first the most anterior region of the alimentary canal, and lies at right angles to the position which it assumes later.

In fact, the trunk of the young fixed Ascidian undergoes an actual rotation through an angle of 90° as the result of which the mouth, which was at first dorsal, becomes terminal, and the endostyle takes up its definite longitudinal position at the base of the branchial sac.

In comparing the accompanying figures (1 and 2) the attention should, first of all, be concentrated on the endostyle, and then the structures which precede and follow it in both cases should be taken into consideration. When that is done, I think the inadmissibility of van Beneden's and Julin's view of the homology of the first pair of gill-slits (primary branchial canals) of Ascidians with the proboscis-cavity and præoral pit of *Amphioxus* (anterior intestinal diverticula) will at once become evident.

In *Clavelina* the behaviour of the proboscis-cavity is essentially the same as in *Ciona.*

Origin of the Gill-slits in Ciona *and* Clavelina.

The post-larval appearance of the gill-slits of the simple Ascidians has been studied to a certain extent only by P. J. van Beneden, Krohn, Kupffer, and Éd. van Beneden and Julin; and most thoroughly by the last two authors. The first three observed young Ascidians with two branchial apertures on each side. Van Beneden and Julin ("Rech. sur le Développement postembryonnaire d'une

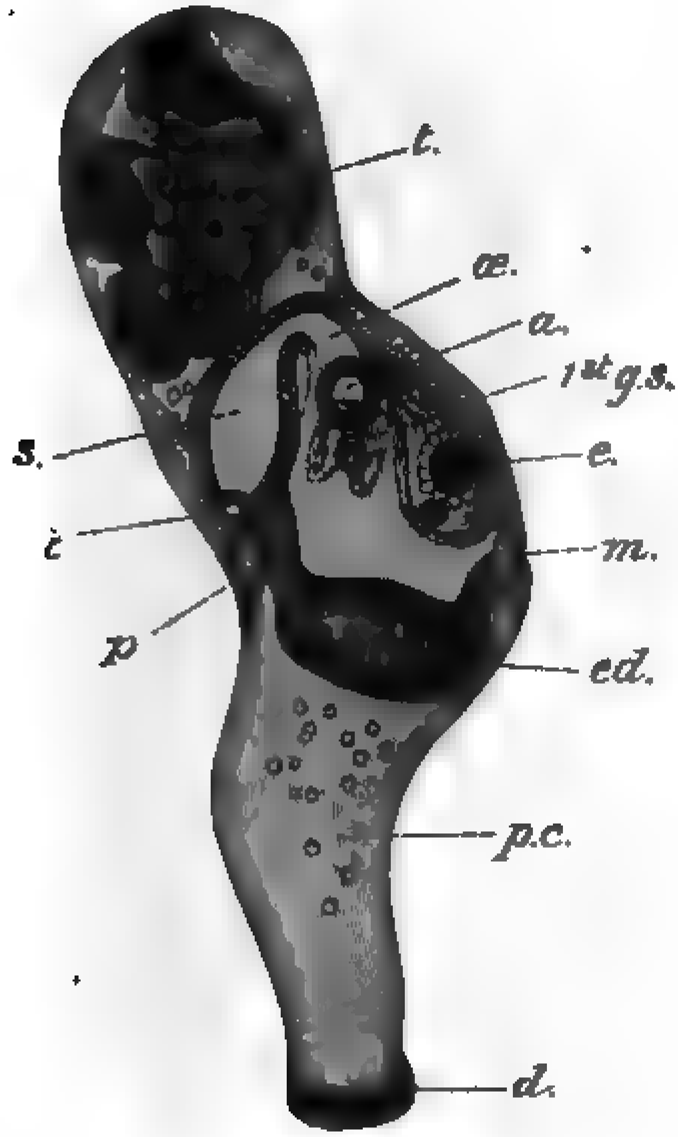

FIG. 1.—A young *Ciona*, shortly after fixation. From the right side. Drawn with cam. luc., Zeiss 3 C.

N.B.—The atrial aperture is merely the external aperture of the first gill-slit.

Explanation of Letters.—*t.*, remains of tail; *œ.*, œsophagus; *a.*, atrial aperture of this side; 1*st g. s.*, first gill-slit (in *Ciona*, double from the beginning); *e.*, eye; *m.*, mouth; *ed.*, endostyle; *p. c.*, proboscis cavity; *d.*, adhering disc; *p.*, pericardium; *i.*, exit of intestine from stomach; *s.*, stomach.

Phallusie," 'Arch. de Biologie,' vol. 5, 1884) commenced with individuals possessing four on each side. In all cases they were supposed to represent gill-slits which had developed by independent perforations.

Van Beneden and Julin, judging from the sizes of the slits, came to the conclusion that they formed in a very irregular manner, the first slit formed being the fourth of the series, and so on. In other words, the true first gill-slit of the Ascidians has been, up to the present, unknown; and it is owing to the ignorance which has hitherto prevailed with regard to this slit that the relations between the Ascidians and *Amphioxus* have been so little understood. Thus,

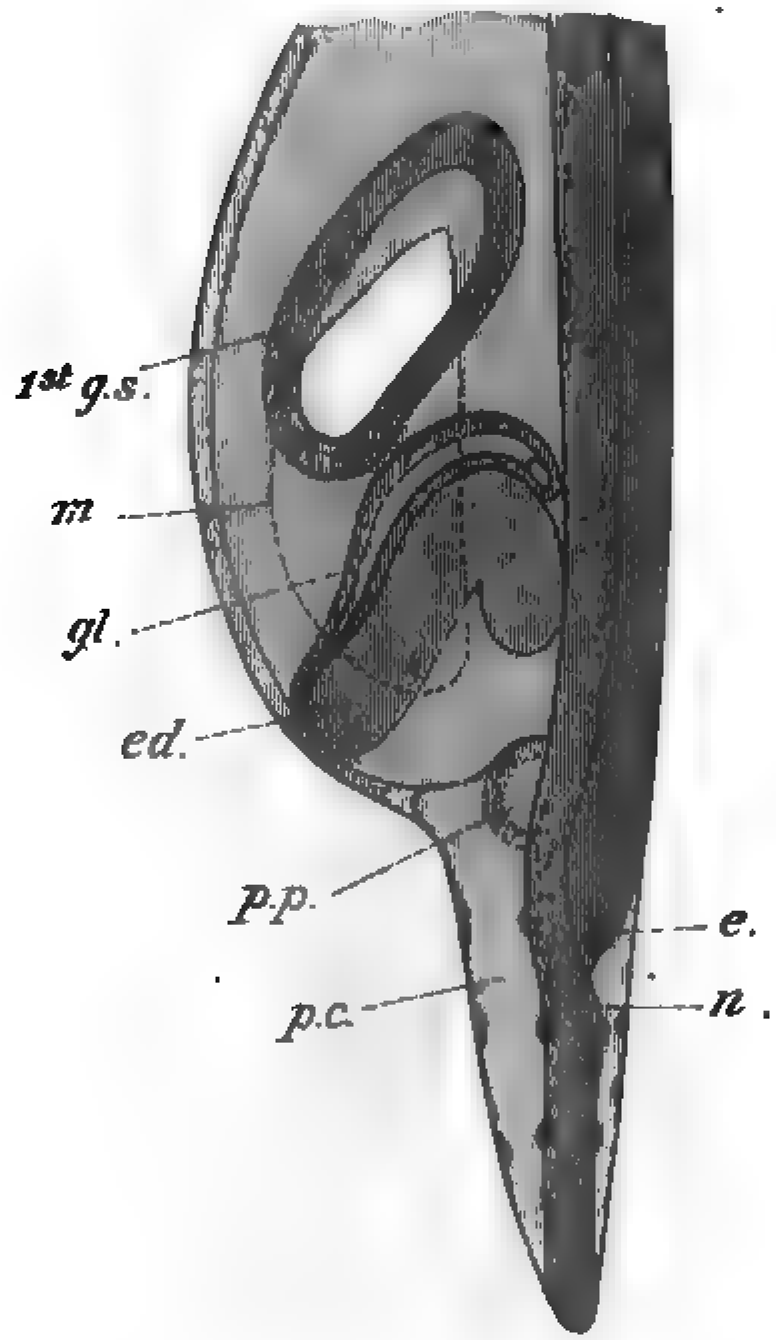

FIG. 2.—Anterior end of young larva of *Amphioxus*. From right side. Præoral pit (*p. p.*) and mouth are seen through.

gl., club-shaped gland; *n*, notochord. Other letters as in fig. 1.

up to the present time there has been no gill-slit described in Ascidians which possessed characters peculiar to itself, and which separated it from the rest. Such a gill-slit, however, exists, and I will now describe it.

Starting with the stage in which two oval apertures are present on each side (in *Ciona*), we find that, as time goes on, these elongate very considerably in the transverse direction, and eventually become twisted round in a curious way at their distal ends (that is, the ends towards the endostyle), and finally a small portion becomes constricted off from the distal end of each of the original slits (fig. 3).

In this way, therefore, we arrive at the stage with four branchial

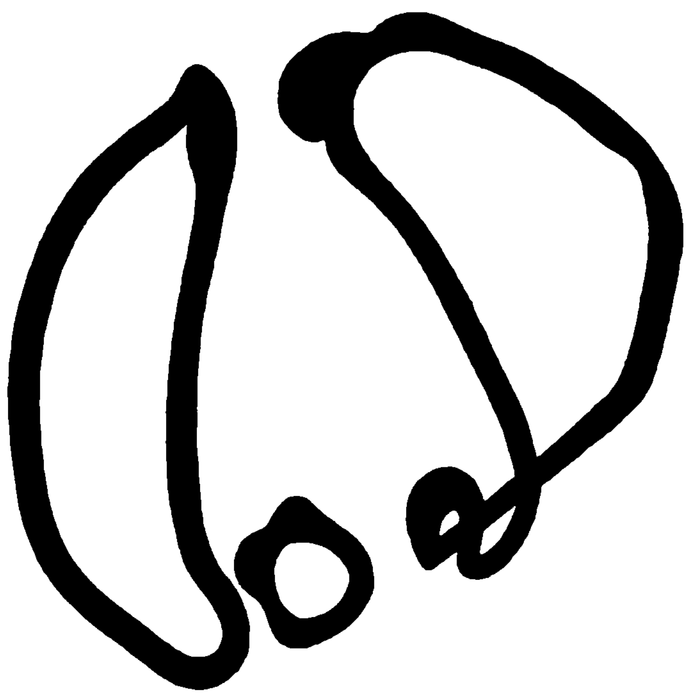

Fig. 3.—Primary branchial apertures of right side of *Ciona*, showing the way in which the stage with four slits on each side becomes established. Drawn from living object. Zeiss 4 B, cam. luc.

stigmata on each side. The slits which form after this (5th and 6th, &c.) arise by independent perforation.

The point is now to determine the origin of the first two slits, and this is by no means easy. By the study of great numbers of living specimens, and more especially of horizontal sections, I have convinced myself that the following is what takes place. It has been known for many years that the slits of *Amphioxus* become each divided into two halves by the formation of a tongue-bar. In the larva, however, the unpaired slits are simple, and remain simple till near the end of the period of metamorphosis. During this period the slits of the second row make their appearance, and very soon afterwards, the primary slits being still simple, tongue-bars begin to form in the secondary slits. Thus in this case the tongue-bars are considerably hastened in their development. If, now, they were hastened a little more, what we should see would be that the two halves of the slit would become independently perforated. *This is actually what occurs in* Ciona. I have very convincing evidence for this point of view, which I hope to produce in detail, accompanied by figures, later. The first four stigmata, therefore, are derived from, and represent, *one gill-slit.*

The primary branchial canals of van Beneden and Julin are simply the first pair of gill-slits. The atrial involutions are at first nothing but the ectodermic portions of a pair of gill-slits. They remain permanently in this condition in Appendicularia; but in the fixed Ascidians they become secondarily expanded to form a distinct chamber. The view of van Beneden and Julin, that the visceral wall of the atrium is endodermic, while the parietal wall is ectodermic, I consider to be unfounded.

The walls of the atrial chamber are apparently derived essentially from the ectoderm.

The first pair of gill-slits which becomes sub-divided in the remarkable way above described (totally different from the transverse sub-division which they subsequently undergo), is present in a simple undivided form in Appendicularia, and I consider it, both from its position and from its specialised character, to be undoubtedly homologous with the first pair of gill-slits in *Amphioxus*, which disappear at the close of the larval period. Both in *Ciona* and in *Amphioxus* this first pair of slits alone serves for the respiration of the larva (or, in the case of *Ciona*, young individual) for several weeks, during which the size of the animal is increased, but no new organs are added. In other words, there is a resting stage in the development of *Amphioxus* and *Ciona*, which is characterised in both cases by the presence of a single pair of gill-slits, namely, the first pair.

By the first pair of gill-slits of *Amphioxus*, I refer to the first gill-slit proper, and to the club-shaped gland. I have on a former occasion given cogent reasons for regarding these two structures as a pair of gill-slits.

In *Ciona*, at a later stage, the primary stigmata, whose origin has just been described, become divided in the usual way and give rise to the transverse rows of stigmata. *In* Clavelina *these transverse rows of stigmata form in the first instance, each aperture being an independent perforation.* Here, then, we have unequivocal evidence of considerable modification in the direction of a shortening of the development in the case of *Clavelina;* and we meet with similar evidence at every turn.

Origin of Pericardium and Heart in Ciona *and* Clavelina.

With regard to the origin of the pericardium, my observations appear to confirm, in the main, the account given by van Beneden and Julin as to its being endodermic; but, as I have never succeeded in finding karyokinetic figures in connexion with its development, a precise statement as to its mode of appearance is not at present possible.

In *Clavelina*, where it is comparatively easy to be persuaded of its endodermic origin, it arises at a much earlier stage in the development of the larva than it does in *Ciona*. In the former it arises before the formation of the body-cavity, while in the latter there is a wide body-cavity present at the time of its first appearance, containing loose mesoderm cells; and the failure to find nuclear spindles, combined with the extraordinarily small size of the object in transverse section, renders it extremely difficult to assign its origin to the endoderm

with certainty, although, from the appearances presented, and also from the analogy of *Clavelina*, it is probable that it arises in the same way in both cases.

The formation of the heart presents interesting differences in the two forms. In *Clavelina*, as already shown by van Beneden and Julin, the septum which, at first, divides the pericardium into two halves breaks down, and the heart forms as an involution of the dorsal wall of the pericardium. In *Ciona* the septum does not break down, and the heart forms by a splitting apart of the two layers which compose the septum.

Conclusions.

What has been said above is enough to show that the development of *Ciona* presents much more primitive features than that of *Clavelina*. It now remains to compare the conditions in *Ciona* with those that obtain in *Amphioxus*, and to seek to establish the true homologies between the various parts.

In *Amphioxus*, what I propose to call the proboscis-cavity is lined by a flat epithelium, and so is the rest of the body-cavity. In *Ciona*, the proboscis-cavity contains loose mesoderm cells in place of an epithelium, and so does the rest of the body-cavity. The distinction between mesoderm and mesenchym is no longer generally recognised as fundamental.

The presence of the præ-oral pit as a pair to the proboscis-cavity of *Amphioxus* seems, at first sight, to present a difficulty in the way of the comparison which I am making; but it is not so serious as might be supposed, and, for the rest, I need only refer here to what occurs in different species of *Balanoglossus*.

In instituting any comparison between the Ascidians and *Amphioxus*, the endostyle should be taken as the starting point, and the fact should be remembered that its primary axis is perpendicular to its definitive axis in both cases.

Making allowance for the secondary change of position which the mouth has undergone in the larva of *Amphioxus*, in correlation with the forward extension of the notochord, we find, therefore, that the relative position of the various organs from before backwards is precisely the same in *Ciona* and in *Amphioxus*, namely, (1) proboscis-cavity, (2) endostyle, (3) mouth, (4) first pair of gill-slits.

It should be remarked that the mesoderm which lies in the proboscis-cavity of *Ciona* has a bilateral origin, corresponding more or less closely to the pair of anterior intestinal diverticula of *Amphioxus*.

It is most important to establish the homology of the cavities of *Ciona* and *Amphioxus* on a sound basis. The endostyle admittedly occupies the same position primarily in both animals.

If, then, the question be asked, "What lies in front of the endostyle?" the immediate response is, "In both cases the proboscis-cavity."

I accordingly submit the following table of homologies:—

(*a*.) Proboscis-cavity of Ascidians	= Proboscis-cavity and præoral pit of *Amphioxus*.
(*b*.) Endostyle of Ascidians	= Endostyle of *Amphioxus*.
(*c*.) Mouth of Ascidians	= Mouth of *Amphioxus*.
(*d*.) First pair of gill-slits of Ascidians, in the improved sense of the term.	= First pair of gill-slits of *Amphioxus*.

The homology of the club-shaped gland of *Amphioxus* with the intestine of Ascidians, as suggested by van Beneden and Julin, would seem, therefore, to be quite out of the question.

It need hardly be pointed out that, if the homologies which I have advanced are really correct, then the relations between *Amphioxus* and the Ascidians become much less strained than they were on the views previously entertained.

I intend shortly to discuss the whole subject more elaborately ir the pages of the 'Quarterly Journal of Microscopical Science.'

VI. "The Human Sacrum." By A. M. PATERSON, M.D., Professor of Anatomy in University College, Dundee, St. Andrews University. Communicated by Professor D. J. CUNNINGHAM, D.Sc., F.R.S. Received April 18, 1892.

(Abstract.)

Owing to the now classical investigations of Gegenbaur and Frenkel, and the more recent researches of other observers, the several homologies of the vertebral column are distinctly understood. The specific or individual differences in the correlation of one region of the column to another can be adequately explained on the assumption of a suppression or excessive development of the potential costal element of the vertebral segment. This costal element may be metamorphosed in different ways to suit the needs of the animal economy, and the variations in individual cases affect the segments at the ends of a series where the vertebræ of one region possess characters resembling those of a neighbouring region. This hypothesis renders intelligible, not only the existence of cervical ribs, but also correlated variations of the thoracico-lumbar region, and abnormalities of the sacrum, differences in the number of bones, as well as asymmetry.

During recent years this aspect of the subject and numerous

examples of abnormalities in the arrangement of the vertebral column have been carefully scrutinised in four important monographs. Rosenberg's memoir has excited the most attention, as he has formulated the theory of a phylogenetic shortening of the human vertebral column from behind forwards. He relies for his conclusions upon the examination of abnormalities of the vertebral column of man and the higher apes, and the statement of Kölliker, that the ilium in the process of development at first articulates with hinder segments, and gradually shifts forwards along the vertebræ to be connected with segments placed more anteriorly. Thus Rosenberg regards a human vertebral column with an increased number of præsacral vertebræ as an "ancestral" form: a column with a diminution in the number of præsacral segments as a "future" form, representing a more recent phylogenetic process. Topinard has recorded a number of observations on vertebral abnormalities. He considers that anomalies in the thoracic and lumbar regions may be due to excess, default, or compensatory variations; that the anomalies of the sacrum are always compensatory, depending partly upon the relation of the ilium to the vertebral column, and partly upon the atrophy and fusion of the caudal vertebræ. He thus gives a qualified support to the notion of intercalation and excalation of vertebræ, as far as the thoracic and lumbar vertebræ are concerned. Regalia and Holl both reject Rosenberg's "atavistic" hypothesis as inadequate. Regalia regards thoracico-lumbar variations as caused by correlated variations in the position and proportions of the thoracic and abdominal viscera; and agrees with previous authors that lumbo-sacral and sacro-caudal abnormalities are due to alterations in the position of the ilium in relation to the vertebral column. Holl, from embryological investigations, considers that the sacrum, once formed, undergoes no alterations, and that the 25th vertebral segment is, as a rule, the first sacral vertebra from the earliest time. He also asserts that the same segment (*v. fulcralis*) has in the great majority of cases in the adult the main attachment of the ilium. He looks upon variations at the cephalic end of the sacrum as caused by changes in the position of the ilium; variations at the caudal end, as associated with fusion of the coccyx.

The present memoir deals with the characteristics of the human sacrum, its form and anomalies, its correlation to other regions of the vertebral column in man and other mammals, its relation to the spinal nervous system, and its ossification, especially in relation to that aspect of the question brought into prominence by Rosenberg's hypothesis. The sacral index and the sacral curve are also dealt with.

The investigations have been made in a series of 265 adult sacra and numerous fœtal vertebral columns. Of the adult sacra, 38

belonged to spines absolutely complete, and 96 to spines complete except for a deficiency of the coccyx. The material has been obtained from many sources: and I am specially indebted to Professor D. J. Cunningham, of Dublin, for the use of a large number of specimens, and notes and drawings of observations made by him on the subject.

The conclusions arrived at are as under:—

1. The examination of a large series of vertebral columns compels one to discard as inadequate the theory of "intercalation" and "excalation" to account for the variations in the number of vertebræ in the several regions. The hypothesis of inherent variability, of shifting of one region at the expense of another, fully explains the cases of individual variation. The changes met with may be regarded as produced, not by the sudden (and anomalous) interposition or loss of a vertebral segment, but by a conversion of the segments of one region into those of another and contiguous region.

2. (*a.*) There is a marked tendency on the part of the first sacral vertebra to be liberated from the rest of the sacrum. It retains its individuality more clearly than the other vertebræ, and frequently approximates in type to the lumbar series.

(*b.*) The surface for articulation with the ilium, while usually placed on the first two, and a part of the third, sacral vertebræ, varies considerably in position. The surface may be shifted backwards or forwards; and the tendency is more marked towards a shifting in the caudal than the cephalic direction.

(*c.*) The surface for attachment of the sacro-iliac ligaments is generally subdivided into two or three depressions, of which that on the first sacral vertebra is, in the great majority of cases, the largest and deepest. The inference from this fact is that the first sacral vertebra has usually the greatest responsibility in supporting the ilium.

(*d.*) In the vast majority of cases there are five constituent bones in the sacrum. Increase to six is much more common than diminution to four; and increase by addition at the caudal end is apparently much more common than by addition at the cephalic end.

(*e.*) Asymmetry of the sacrum occurs frequently (8·3 per cent.). It occurs in two forms: as either a sacro-coccygeal or a lumbo-sacral vertebra; and in two ways, by diminution or addition at either end in the number of component bones. A sacro-coccygeal vertebra is more frequent than a lumbo-sacral; and asymmetry with addition is more common than asymmetry with diminution in the number of bones forming the sacrum.

(*f.*) The examples of correlated variations of the several regions of the vertebral column indicate a greater tendency towards increase than diminution in the total number of bones. Increase is more common than diminution in the number of bones in the præsacral, sacral, and

caudal regions respectively. Increase in the sacral region is more common by abstraction from the caudal than from the lumbar series. Liberation of the first sacral vertebra is more common than assimilation of the fifth lumbar vertebra; and assimilation of the first caudal vertebra is more common than liberation of the fifth sacral. With regard to the sacrum particularly, there is found to be a certain limited and inherent variability in the position of the ilium, causing it to be shifted backwards or forwards in relation to the vertebral axis, and more frequently backwards than forwards. There appear to be three separate influences acting upon the sacrum, and producing the differences in number of bones, correlated variations, and asymmetry:—(1) fusion of the first caudal vertebra; (2) liberation of the first sacral vertebra, by a backward shifting of the ilium, along the vertebral axis; and (3) fusion of the last lumbar vertebra with the sacrum, by a forward shifting of the iliac attachment. The first influence is most commonly seen, and may be exerted alone or along with the second. The second and third influences are opposed to one another. The former is more frequent than the latter, producing an additional lumbar, or a lumbo-sacral vertebra; the latter gives rise to a diminution in the number of free lumbar vertebræ, and may be accompanied by the conversion of the last sacral into a sacro-caudal or caudal vertebra.

(*g.*) A study of the ossification of the vertebral column leads to similar conclusions, and indicates the existence of inherent variability in the several regions, and a greater tendency to elongation than contraction of the vertebral column as a whole. The process of ossification also shows that the ala of the first sacral vertebra (25th spinal segment) is usually the first to ossify; which vertebra may therefore be regarded as the one primarily responsible for the attachment of the ilium. The exceptional cases occur in sacra showing correlated variations or asymmetry, and indicate a greater tendency on the part of the ilium to be shifted backwards than forwards.

(*h.*) The evidence derived from a consideration of the vertebral column in other vertebrates is unsatisfactory. The human spine holds, with regard to correlated variations, a position intermediate between anthropoid apes (in which they are very frequent) and quadrupeds generally (in which they are rarely present); while asymmetry, especially of the sacrum, may be looked upon as an essentially human characteristic.

(*i.*) The examination of the correlation of the spinal nerves and limb-plexuses with the vertebral segments shows both specific and individual differences in this respect. The individual differences may be classified under three types:—(1) a variation in the arrangement of the nerves without any concomitant variation in the vertebral column; (2) a variation in the vertebral column without any con-

comitant variation in the nerves; and (3) a coincident variation in both nervous system and vertebral column. These varying relations of the nervous system to the vertebral column diminish the value of the spinal nerves in the determination of the serial homologies of the vertebral segments. Further information as to the relative frequency of abnormalities in the disposition of the spinal nerves in the limb-plexuses and the relative frequency of the various modes of correlation of the spinal nerves and vertebral column is required before adequate conclusions can be formulated on this point. One can only indicate the striking fact that all the examples of variation in the arrangement of the spinal nerves in man and anthropoids are examples pointing to an extension *backwards* of the limb-plexuses, by the entrance into them of post-axially placed nerves. The evidence of the nervous system favours, therefore, the view rather of extension backwards than forwards of the limbs in relation to the vertebral column, as far as present knowledge enables us to judge.

When all these points are considered together—the cases of liberation of the first sacral vertebra, the mode of articulation of the ilium; the form of the ligamentous surface and the number of bones forming the sacrum; the correlated variations; the examples of asymmetry; and the evidence derived from a consideration of the vertebral column in other vertebrates, of the correlation of the nervous system and the vertebral column, and of the development and ossification of the vertebral column in man—the array of facts presented does not afford much support to the theory of a phylogenetic shortening of the vertebral column advocated by Rosenberg. The conclusion to which these facts lead is rather that there is in the human vertebral column a certain limited variability in the correlation of the several parts. The actual variations met with may be in the direction of elongation or abbreviation of a particular region, and more often produce elongation than contraction of the præsacral region. There is no evidence to show that any definite process of either shortening or lengthening of the vertebral column is going on phylogenetically. The variations found are apparently individual peculiarities; which, however, taken together, indicate a tendency to elongation rather than contraction of the præsacral region.

At the same time, these investigations give support to the view put forward by Rosenberg and Topinard, that a process of fusion of the rudimentary caudal vertebræ with the sacrum is going on, in consequence of the immobility of the caudal appendage, and resulting in an increase posteriorly of the number of sacral vertebræ, and a diminution, *pari passu*, in the number of free caudal vertebræ.

3. The sacral index was computed in a large number of cases, including 100 Europeans, 20 Andamanese, 9 Negroes, 10 ancient

Egyptians, 5 Hindoos, 8 Australians, and a small number of examples of several other races. The results obtained were compared with those given by Professor Sir William Turner in his monograph on the Human Crania and Bones of the Skeleton, collected during the voyage of H.M.S. "Challenger."

The mean index calculated from the two series of observations, disregarding sex, is 106·7; of the males alone, 103·5. The human sacrum is thus generally broader than long, as already known; and the female sacrum is relatively broader than the male.

The races dealt with may be divided into three classes:—(*a.*) Those distinctly *dolichohieric*, with a sacral index below 100, including the Kaffir, Hottentot, and Bushman. (*b.*) Those which may be called *sub-platyhieric*, with an index between 100 and 106, including the Andamanese, Australian, Chinese, Tasmanian, and Negro races. (*c.*) Those which are clearly *platyhieric*, with an index over 106, including American, ancient Egyptian, Melanesian and Polynesian, Hindoo, European, and other races examined.

4. The sacral curvature was examined in 236 cases, of which 82 were males, 38 females, and the rest of unknown sex. The curve is generally deepest opposite the third sacral vertebra, and is more deeply curved below than above that point. It is not an equal and uniform curve, but is flattened above, and possesses a more pronounced curvature below the third sacral vertebra. This occurs in both sexes and in all races. A promontory between the first and second sacral vertebræ occurs frequently, in the majority of cases in association with an additional sacral vertebra, and more often in the male than in the female.

The female sacrum is more frequently curved more deeply in the upper part of the bone than the male sacrum.

The actual depth of the curve, that is, the amount of curvature, is greater in the male than in the female, irrespective of the absolute *size* of the sacrum. The amount of curvature is greatest in the European races, and, apparently, greater in the European and Mongolian races than in the Negro and Polynesian.

The Society adjourned over Ascension Day to Thursday, June 2.

Presents, May 19, 1892.

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OBITUARY NOTICES OF FELLOWS DECEASED.

SIR GEORGE AIRY was born on July 27, 1801, and died on January 2, 1892. It is not a possible task to compress into a few pages the ninety years' work of a great man; all that can be done is to indicate a few of his many achievements. When his life is written it will be a book and not a pamphlet, and only then shall we understand how much of our scientific knowledge we owe to him. The number of articles and memoirs which he has communicated to the various Societies and journals in which he was interested are over five hundred in number. The first of these is a paper read to the Cambridge Philosophical Society, on November 25, 1822, on the use of silvered glass for the mirrors of reflecting telescopes, and the last is his Numerical Lunar Theory. The date of the first paper is particularly interesting, for it is the year *before* he took his degree. The last paper is also remarkable, for, remembering that the theory of the Moon is one to which some mathematicians have devoted nearly their whole lives, it shows the old man attacking a laborious problem with the energy of youth.

Sir George was educated at private schools at Hereford and Colchester. His vacations from school were spent at the Hill Farm, near Playford, with the uncle by whose assistance he was enabled to go to Cambridge. He never appears to have played cricket, or football, or rowed, but he delighted in pedestrianism. We are told that, with a companion, he once attempted to walk from Playford to Bury St. Edmunds and back in the same day. They reached Rushmere Church on their way home, but could not do the last mile and a half, and the journey had to be finished in a cart sent to meet them.

In 1819 he entered Trinity College, as a sizar, but it does not appear that he was elected a scholar until he was in his third year. In 1823 he became Senior Wrangler and First Smith's Prizeman. The writer can remember that when, thirty years after that date, he entered Cambridge, the story was still told amongst the undergraduates of how wonderfully Airy had answered in the examination. Possibly nothing had been lost in the telling, but there must have been some extraordinary excellence to have attracted such long continued attention.

As soon as he had taken his degree be began a life of ceaseless scientific activity. Memoirs followed each other with ever increasing rapidity, each bearing evidence of much thought and of considerable

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work. The subjects also were of the most varied kinds and on all parts of mathematical philosophy. At first he wrote chiefly for the Cambridge Philosophical Society; thus in March, 1824, he calculates the effect which the ring of Saturn produces on his figure. He tries to verify the curious observations of Herschel that this planet is protuberant between the poles and the equator, but he finds that theory leads to an exactly opposite result. The observations of Herschel were, however, considerably modified by those of Bessel some years after. In another paper in the same year he discusses achromatic eye-pieces; for this and his other papers on optical subjects, the Copley Medal of the Royal Society was adjudged to him in 1831. Soon after, he writes on the proper forms of the teeth of wheels, though, owing to the extensive use of iron where wood was formerly used, this subject has no longer the interest it once had.

One of the most interesting papers written by him, about the year 1825, is on a peculiar defect of his own eye and the mode of correcting it. He discovered that in reading he did not use his left eye. Supposing this to be due to habit, he endeavoured to read with the right eye shaded, but found he could not distinguish a letter at whatever distance from the eye the characters were placed. Some time after he made a further discovery, viz., that the image of a point formed by that eye was not circular but elliptical. From this and other appearances he inferred that the refraction of the eye was greater in the vertical plane than in that at right angles. To correct this it would be necessary to use a lens which would refract more powerfully the rays in one plane than those in the perpendicular plane. His idea was that the lens should have one surface cylindrical and the other spherical, and he describes at length his experiments to determine the proper radii. The result was so successful that he was able to read the smallest print at a considerable distance with the left eye as well as with the right. In his subsequent papers he frequently returns to this subject and makes several reports to the Society on the changes produced in his eye by lapse of time.

In 1826, when Ivory criticised Laplace, Mr. Airy, in a paper contributed to the Cambridge Philosophical Society, was not afraid to intervene between two such distinguished analysts when he thought that both had gone wrong. Mr. Airy was not indisposed to controversy; possibly it added a touch of life to his science. We find him afterwards engaged in many disputes, in all of which he was able to prove that he was a tough adversary. In this year, three years after his degree and ten years before he became F.R.S., he read his first paper before the Royal Society. The subject was the much debated question of the figure of the Earth. Alluding in it to the peculiar views of Ivory of fluid equilibrium, he was attacked by that mathematician in a somewhat arrogant manner. This called forth from Mr. Airy a

crushing reply, which he published in the 'Philosophical Magazine' in 1827.

It is, however, impossible even to mention the names of the many papers of which he was the author. The few which have been mentioned above prove how soon after his degree he took a leading part in the scientific work of the day. They show also how, from the very beginning, his mind was turned to practical applications, leaving aside any pure theorems of which he did not see the immediate use.

In 1826 Mr. Airy published his mathematical tracts, which almost immediately became the standard text-book for students in the University. In the first edition we find only the lunar theory, the figure of the Earth, precession and the calculus of variations, the tract on the planetary theory and that on the undulatory theory being added in the second edition, 1831. As his object was to give a clear statement of first principles, he put into the book just what was wanted at the time he wrote, making his judgment with admirable skill. The student world has now outgrown the book, but this is in part due to the excellence of the teaching of the book itself. The first tract, that on lunar theory, is interesting to Cambridge students for another reason. The attention of the University had been so long confined to the works of Newton that the analytical mode of treatment had been almost entirely neglected. The methods of Newton are, Mr. Airy remarks, beautiful, but they have all the imperfections which necessarily accompany first attempts; for the explanation of some of the lunar inequalities they are hardly sufficient, and for the calculation of most they are quite inadequate. For other branches of physical astronomy, such as the planetary theory, their inadequacy has never been questioned. In this tract he endeavours to lay before the student an analytical view of the lunar theory, giving references to the 'Principia' to show the connexion between the different systems. The tract on the calculus of variations is the only one which is purely mathematical. Though it does not go very far into the subject, yet the author must have had a deep sense of the power of this calculus, for he has used it in his physical papers, even in places where simpler methods might more naturally have suggested themselves to his mind. In the preface he speaks of this calculus as the most beautiful of all the branches of the differential calculus. The excellence of the tract on the undulatory theory is evident when we remember the length of time in which it was regarded as the standard text-book of the University. When the other tracts, after a long life, became antiquated, this one retained its popularity, and has been reprinted several times by itself, and is even yet in use.

Mr. Airy was elected a Fellow of Trinity College the year after his degree, and later on in life he was chosen one of the three first Honorary Fellows of the College, the others being Thirlwall and

Tennyson. In 1826 he was appointed Lucasian Professor of Mathematics, but this professorship he soon exchanged for the Plumian, to which he was appointed in 1828. According to the Calendar of that date, his predecessor in office merely gave lectures in the first half of the midsummer term, while those of the former Lucasian Professor are only vaguely referred to. But these were greatly enlarged by Mr. Airy, whose syllabus extends over forty-eight pages of print. They comprise statics, dynamics, hydrostatics, and geometrical optics, but their chief character seems to have been the theory of undulations. Many of the experiments on polarised light whose mathematical theory is given in his tract on the undulatory theory were exhibited here. He appears to have been the first to introduce into Cambridge studies the beautiful theories of Fresnel. With these as subjects, treated in his own skilful manner, we need not wonder at the popularity of his lectures. Even after he had become Astronomer Royal, we learn from his first report to the Board of Visitors, that application to the Admiralty had been made by several members of the University and by the Plumian Professor to allow him to give another course of lectures at Cambridge.

Along with the Plumian Professorship Mr. Airy undertook the duties of the Director of the Observatory. He at once entered on these arduous duties with his usual energy. His efforts were well seconded by the University, who at once raised the slender income of the professorship to an amount nearly double its former value. In the first volume of the 'Astronomical Observations' he tells us that he was induced to fix on a plan of publication very different from that of the 'Greenwich Observations.' He remarks that the value of unreduced observations is so small that to most persons they are absolutely useless. Few, who have not made observations, understand how much time and calculation must be employed before they can be applied to any useful purpose. On the average, the preparatory steps and the observation of a transit occupy from five to ten minutes, while the complete reduction and discussion of the observations employ full half an hour. The professor even said that if an offer was made of a mass of regular meridional observations unreduced, he would not think it worth acceptance. In giving, therefore, the results, he was giving the produce of four or five times as much labour, necessarily irksome, as if he gave merely the unreduced observations. The report for the year 1828 covered the interval of five months' residence at the Observatory; he had no assistant, and every step from making the observations to revising the proof-sheets had to be done by himself alone. Yet in April of the following year the report was published with all the necessary reductions. This promptness is maintained in the succeeding years, and excited the admiration of M. Quetelet, the Director of the Obser-

vatory at Brussels, who says, "Nous sommes à peine au milieu de 1832, et déjà nous possédons les observations de M. Airy, pour toute l'année 1831: et ce qui peut paraître plus étonnant encore, toutes ces observations sont calculées et discutées avec soin."

It is interesting to observe the care with which he chose the objects to which he should turn his attention as an astronomer, and the constancy with which he stuck to his choice when once made. The chief object, he says, must be such that it could be accomplished by a single unassisted observer, and yet be so important as to be of public use. After consideration he decided that the observations of planets had at that time been so neglected, that one who wished to revise the planetary tables would find himself destitute of the necessary data on which to found his investigation. As soon, therefore, as the Cambridge Observatory was placed under his direction, he made the observation of planets the leading object of his labours. He says in one of his reports that "hardly a single observation of a planet has been lost when the transit was at such an hour that in the regular routine of observations it was practicable to observe it." The wisdom of his choice is shown by the fact that his successor followed closely the same objects. Other pressing wants in astronomy were also present in his mind, and others again rose unexpectedly in the course of his work. In reading his yearly volumes of observations, one notices among other things the care which is taken to secure accuracy. No labour is spared, no calculation is allowed to pass without repeated examination. "To observe all night and to calculate all day" is the description of an astronomer's duties given by an astronomer. In the arrangement of his results, we notice, also, how everything is subordinated to increasing their immediate utility as well as securing accuracy in their details.

When Professor Airy first went to the Observatory the only large instrument was a transit, though this was one of the best of its kind. So energetic an astronomer was not likely to be satisfied with this; accordingly in 1834 he obtained a large mural circle. In the report for that year he describes the unexpected and annoying difficulties which arose in connexion with that instrument. In the next report we find that these difficulties have been overcome by considering that the effects of the discordance of zenith points on direct and reflexion observations are equal. Later on the great Northumberland equatoreal was added. The establishment to work these was also necessarily increased, and two assistants were given to him.

Perhaps one of the most remarkable examples of Mr. Airy's insight into astronomical questions is his discovery of a new inequality in the motions of Venus and of the Earth. The attention of the Board of Longitude having been directed to the state of the solar tables used in the construction of the 'Nautical Almanac,' he was desired to

examine the discrepancies between the computed right ascensions of the Sun and those observed at Greenwich. On making a comparison between the discrepancies in the position of the Sun's perigee as given by late observations with those given by the observations of the last century, he concludes there must be some yet undiscovered inequality which has been omitted from the calculations. He soon discovered that this originated in the fact that thirteen times the periodic time of Venus is so nearly equal to eight years that the term depending on this phase received a multiplier of more than two millions in integrating the differential equations. On the other hand, the coefficient is of the fifth order with regard to the eccentricities and inclinations of the orbits. In the report on this paper drawn up by Whewell and Lubbock for the Royal Society, it is pointed out that no numerical calculation of a perturbation of the fifth order had been executed, except in the case of Jupiter and Saturn, where, as Laplace states, this labour, "pénible par son excessive longueur," had been performed by Burckhardt; and no calculations of a new inequality of a high order, requiring to be placed in the planetary tables, with a new argument, had been published since that of the great inequality by Laplace in 1784. They conclude by remarking that this is the first specific improvement in the solar tables made by an Englishman since the time of Halley. For this brilliant investigation the Astronomical Society in 1833 awarded to its author their gold medal. The whole of Professor Airy's process was afterwards verified, first by Pontécoulant, and secondly by Leverrier, and found to be correct.

In the years 1831–32, Professor Airy, though so fully employed at the Observatory, was yet able to make some important investigations in the theory of light. Thus he communicates to the Cambridge Philosophical Society a paper to show that the two rays produced by the double refraction of quartz are elliptically polarised. This is soon followed by two or three papers on some phenomena connected with Newton's rings. Just as Sir W. Hamilton afterwards predicted internal and external conical refraction after studying the analytical properties of the wave surface, so Professor Airy discovered these phenomena by using Fresnel's general formula for the intensity of reflected light. When Newton's rings are formed by light polarised in a plane perpendicular to that of incidence between two substances of different refractive indices, and the angle of incidence lies between the polarising angles, the rings should appear white centred, instead of having a central dark spot. Here was a recondite phenomenon which could only be seen when several special conditions were satisfied. Would it be confirmed by experiment? He describes the difficulties of the experiment and its final success. As we read the paper, we perceive how he is led on by slight unexpected discrepancies to

improve the theory. He remarks that there must be a gradual, though rapid, change of phase, instead of the sudden one given by Fresnel's formula, thus seeing faintly a result clearly explained five years after by the theoretical investigations of Green.

At this period of his life Professor Airy's labours are evidently divided between astronomy and the theory of light. The first was connected with his work at the Observatory, the second with his lectures as Professor. Thus, in 1833, he writes in the 'Cambridge Transactions' on Newton's experiments in diffraction; in 1835, on the diffraction of an object glass with a circular aperture; in 1838, on the intensity of light in the neighbourhood of a caustic. In 1840 he chose as the subject of the Bakerian Lecture the theoretical explanation of an apparent new polarity in light.

There is an equally important list of papers on astronomy. In 1832 he communicates to the British Association a report on the progress of astronomy during the present century. This was translated into German, three years after, by C. L. Littrow, of the Royal Observatory, Vienna. The Viennese astronomer thinks that Professor Airy has treated German astronomy like a step-mother, but, nevertheless, he says there is no other work in which the progress of astronomy is so briefly and so accurately given. In 1834 he writes for the 'Nautical Almanac,' on the perturbations of small planets and comets of short period. There is more than one paper on the mass of Jupiter. In 1834 he writes a paper, for the Astronomical Society, on the solar eclipse of July 16, 1833, which was seen extremely well at Cambridge. On this occasion he adopted a new plan of observation; instead of noting the times of the beginning or the end, he so chose the quantities to be measured that any errors in the elements would be observed after they had been largely multiplied. For example, at the beginning of the eclipse, when the discs of the Sun and Moon only slightly overlap, it is obvious that the length of the straight line joining the cusps is much more affected than the versine by any small error in the angular distance of the centres of the discs. To detect such errors, the attention of the observer should be directed to the length of this line. In like manner, the whole duration of the eclipse was divided into periods, for each of which he arranged appropriate measures.

These papers, too numerous to catalogue in this place, did not exhaust the energy of the Professor, for he found time to publish treatises on Trigonometry, the Figure of the Earth, and one on Gravitation. The latter was written for the 'Penny Cyclopædia,' but previously published, in 1834, for the use of students in the University of Cambridge. It was an attempt to explain the perturbations of the solar system without introducing an algebraic symbol. Having thus denied himself the use of the most powerful

engine of mathematics, he expresses his surprise at finding that a satisfactory explanation could be offered for almost every inequality recognised as sensible in works on physical astronomy. The book, though well received, was, for many reasons, not so popular as his tracts. In 1884, however, it received the honour of a second edition.

In 1836–37 he was President of the Astronomical Society. In the first of these years, when presenting the medal of the Society to Herschel for his catalogue of nebulæ and clusters of stars, he gave an interesting account of the history and of the then state of our knowledge of these curious bodies. The next year the address was on the perturbations of comets.

The year 1835 was a great epoch in Mr. Airy's life, for he was then appointed Astronomer Royal. How thoroughly he intended to work the National Observatory is evident from his very first report, for here we find traces of the reforms he intended to introduce; the arrangement of the volumes of observation was to be remodelled; the library improved; a new equatoreal was suggested; a magnetic apparatus had already been acquired, and the site of a magnetic observatory chosen.

Remembering the views he had expressed on unreduced observations when he began work at the Cambridge Observatory, we naturally inquire what he did with the vast mass of ancient observations which he found unreduced when he arrived at Greenwich. This we learn gradually as we read his reports to the Board of Visitors. In 1841 the observations of planets from 1750 to 1830 had already been reduced to longitude and latitude, and every one had been compared with the place computed from the best modern tables. Sufficient time had not yet elapsed to allow of the reduction of the lunar observations, for here 8000 places of the Moon had to be deduced from observation, and 8000 places had to be computed in duplicate from tables exhibiting the complicated results of the most advanced modern theory. In extent and in importance this work may be considered comparable to any that has yet been undertaken in astronomy. In 1846 these lunar reductions were entirely completed. One immediate result was that Hansen discovered two inequalities of long period in the Moon's motion, produced by the attraction of Venus, though some doubts were afterwards thrown on one of these by Delaunay and Newcomb. For these reductions he received in 1846 a gold medal, and in 1848 a testimonial, from the Astronomical Society. Sir John Herschel, in the latter of these years, after noting that this work will remain to the latest posterity a monument of national glory, remarked that we owe to other nations, and especially to the French, the filling up of the great outline struck by Newton with the analytical expressions of the laws of lunar and planetary motion. This glory, he says, they have fairly won, and it is theirs.

"But the broad basis of observations upon which this magnificent superstructure has been reared is British; in the National Observatory it was created. Such has been the mission of that establishment, and such Mr. Airy has wisely judged it must continue to be, to furnish now, and in all future time, in an unbroken series, the best and most perfect data by which the laws of the lunar and planetary movements, as developed by theory, can be compared with observation."

In the report for 1841 he also describes how the Magnetic and Meteorological Department had grown into an important branch of the Observatory. He tells us that the regular work of the establishment is to observe the meridional, bifilar, and horizontal needles, the barometer and thermometers, besides several other instruments, every two hours night and day, except on Sundays; to pursue incessantly the magnetic observations whenever anything unusual occurs; to observe some of the instruments every five minutes during twenty-four hours on a fixed day every month. As the observations, when made, had all to be reduced and tabulated in proper shape, it is clear that the amount of work done must have been very great. Some of these troublesome observations were afterwards abbreviated by a system of self-registration by photography. The description of this new system is given in the volume of 'Greenwich Observations' for 1847.

In April, 1839, Mr. Airy read to the Royal Society his first paper on the correction of compasses. Captain Flinders, in his famous voyage to Australia, had observed that the north end of his compass appeared to be drawn towards the bows of his ship; and he, and others after him, had suggested methods of compensating the cause of the disturbance. The Astronomer Royal was, however, the first to make a thorough investigation of the laws of magnetic disturbance. The iron ship "Rainbow" was placed at his disposal with a view of discovering by experiment some method of controlling the strange deflections of the compass. The use that he made of this vessel will make its name as famous in the history of the mariner's compass as Stephenson's "Rocket" is in the history of locomotives. Assuming that every particle of iron in the ship is, by the action of terrestrial magnetism, converted into a magnet, he calculated the resolved forces on one end of a compass needle whose centre holds a fixed position in the ship. He found that these forces contained two sets of terms, which he called the semi-circular and quadrantal variations, their phases being respectively the azimuth and twice the azimuth of the ship. The latter of these he found to be due to the induced magnetism; while in the former the permanent, the sub-permanent, and the induced magnetism had shares. Having determined the coefficients of these variations by observing the times of vibration of a delicate needle placed first on shore and then on the ship, he was

able to compare the actual and calculated deviations of the compass from the north for all azimuths of the ship. He soon found that almost the whole deviation of the compass was accounted for by the permanent magnetism, and that the residual part followed nearly the quadrantal law. He thence deduced a simple rule by which the compasses could be practically corrected to a first approximation. Putting the ship's length (1) north and south, and (2) east and west, he showed how to place two permanent magnets near the compass so that it indicated true magnetic north in each position. Placing next the ship's length north-east and south-west, the effect of the quadrantal deviation became prominent, and this he corrected by a mass of soft iron, whose own induced magnetism, when properly placed, counterbalanced that of the ship. These corrections being disturbed when the ship heeled over, another magnet was added. On applying this method to the "Rainbow," and trying the compass with the ship's head in different positions through the circumference, it was sensibly perfect; the deviation, which at first had exceeded 50°, sometimes on one side and sometimes on the other, was at once reduced to half a degree. The great development of iron-built ships soon rendered some modification in these corrections necessary; improvements were made by their author, and other mathematicians also made a special study of the deviations of the compass. Mr. Airy wrote several other papers in connexion with this subject, such as those in 1840, 1856, 1860, and 1862, and in 1865 he delivered a course of three lectures at the School of Naval Architecture and Marine Engineering at South Kensington. The principle that the compass ought be corrected by magnets or otherwise has not been universally received; it was contended that it was better to use a table of errors. The Astronomer Royal maintained that the former course was the proper one, while Mr. Archibald Smith has been the champion of the latter. The question has been much discussed, but cannot be entered into here.

Mr. Airy formed one of an important Commission for the restoration of the standards of weights and measures which had been injured by the fire at the House of Commons. Contrary to the opinion prevalent in France, the Commission recommended that the standard measure should be defined by the length of a certain rod preserved in some place of safety, and not by any natural standard, such as the length of the seconds pendulum. Contrary, also, to the method adopted by Bessel for the Prussian standard, the yard is defined by the distance between two points marked on the bar and not by the length of the bar. The history of standards is given by Mr. Airy in a long paper in the 'Phil. Trans.' for 1857.

About 1841 Mr. Airy turned his attention to the theory of tides. He wrote several papers on this subject, discussing separately the

tides in the Thames, at Ipswich, Southampton, the coast of Ireland, and, later on, the tides at Malta. A Royal Medal was adjudged to him by the Royal Society in 1845 for his inquiry into the laws of the tides on the coast of Ireland. His chief work on this subject is his essay on "Tides and Waves," printed in the 'Encyclopædia Metropolitana.' Taking the usual division of the theory into three parts, viz., the equilibrium theory, that of ocean tides, and that of river tides, we may ascribe the initial steps in the first to Newton and Bernoulli, those in the second to Laplace, while the last may be said to have begun with Airy. This important paper, perhaps because it had not been published by any learned society, did not attract the attention it deserved on the Continent, but its merits could not remain unnoticed, and in 1875 the section on river tides was translated and printed in 'Liouville's Journal.' In this section he discusses the broken water seen on the edge of a shoal, why the rise of tide occupies less time than the fall, the solitary wave, the breaking of waves, the effect of the wind, tidal waves, the effect of friction, the form of the wave in broad channels with shallow sides, and other interesting questions. When M. Delaunay, in 1866, suggested that a portion of the apparent acceleration of the Moon was due to a real retardation of the rotation of the Earth caused by tidal friction, Mr. Airy gave a general explanation, founded on the theory of river tides. He discovered two terms of the second order in his equations whose general effect was to produce a constant acceleration of the waters in the direction of the Moon's apparent diurnal course. He therefore gave his entire assent to the views of Delaunay on the existence of one real cause for the retardation of the Earth's rotation. Other causes of retardation have been discovered by mathematicians since then, but these, of course, lie outside the scope of the present sketch.

In the summer of 1844 the arc of longitude between Greenwich and the island of Valentia was measured. As an intermediate station, the longitude of Kingstown was also determined. The difference of longitude was found by making thirty pocket chronometers travel from Greenwich to Kingstown and back again several times; the difference between Kingstown and Valentia being found in a similar manner. The differences in longitude having been found, the next step was to compare the results with the data of the trigonometrical survey, and to see how far they agreed with the best existing determination of the figure of the Earth. The triangulation was then only partially completed, but enough had been done to enable Mr. Airy to arrive at the result that in the latitude of 51° 40′ the length of 1″ in an arc perpendicular to the meridian is 101·6499 feet in terms of the standard bar of the Ordnance Survey. In the same year Struve determined the difference of the longitudes of Altona and Greenwich by the transmission of forty-two chronometers across the German Sea

sixteen times. In the summer of 1862 the longitude of Valentia was again determined, this time by the use of the electric telegraph. The electric telegraph was also used in 1853 to determine the differences between the longitudes of Greenwich, Cambridge, and Edinburgh, and, when the submarine telegraph was laid to Ostend, the difference of longitude between Greenwich and Brussels. These differences of longitude have been combined with other Continental determinations, and, by the use of the whole series, the longitudes of places in the extreme east of Europe may be compared with places in America.

In the year 1844 Mr. Airy also assisted in tracing the Canadian boundary under the Treaty of Washington. The corps of Royal Engineers who were to mark the boundary were placed at the Observatory for instruction and practice in the use of instruments under his eye. The most difficult part of the boundary was a straight line of nearly seventy miles in length, passing through a country of impervious forests, steep ravines, and dismal swamps. He arranged a plan of operations founded on a determination of the absolute latitudes and the differences of longitudes of the two extremities. The azimuths of the line for the two ends were then computed, and marks laid off for starting from each end. One party of engineers, after cutting more than forty-two miles through the woods were surprised, on the brow of a hill, at seeing a gap in the woods, on the next line of hill; this turned out to be the line of the opposite party. On continuing the lines until they passed abreast of each other, their distance was found to be 341 feet. This corresponds to an error of only a quarter of a second of time in the difference of longitudes, and is about one-third of the error which would have been committed if the spheroidal form of the earth had been neglected. This is a striking testimony both to the accuracy of Mr. Airy's method and to the skill of the engineers.

At a special meeting of the Board of Visitors in 1843, Mr. Airy proposed to construct the first of the important instruments he has added to the Observatory. He points out that the Royal Observatory was instituted chiefly to observe the Moon, and that this object had been so continuously kept in view ever since its foundation, that the existing theories and tables of the Moon are founded entirely on Greenwich observations. The unavoidable interruptions to the regularity of the series were, however, so numerous, that the number of complete observations then made was under a hundred a year. He proposed to supply this deficiency by erecting an altitude and azimuth instrument, by which the Moon could be observed in any part of the sky. He assures the Visitors that its cost ought not to be an objection when it is remembered that each complete lunar observation was then worth ten pounds. In the report of 1847 we read that the new instrument had been completed, and was in working

order. In 1848 he proposed to erect a transit circle. Other great instruments followed, and in 1855, when the want of an equatoreal was pointed out, the Board of Visitors warmly supported his views. In the summer of 1860 the instrument was in a state fit for use Enough has now been said to show that by his energy and perseverance the Royal Observatory has been equipped with the most admirable instruments which could be obtained. As early as December, 1849, the Astronomer Royal made an oral statement to the Astronomical Society on the method of observing and recording transits lately introduced in America. He explains how a measure of its accuracy, as compared with Greenwich observations, had been made, and, after pointing out some defects, he thought the possible advantages so great that he contemplated adopting it at the Royal Observatory. In this report for 1854 he tells us that the new barrel-apparatus had been practically brought into use, not, however, without a succession of difficulties, whose causes it was sometimes very hard to discover. He concludes by remarking that the method is troublesome in use, and consumes much time in preparation, but that, amongst the observers who use it, there is only one opinion on its astronomical merits, viz., that, in freedom from personal equation and in general accuracy, it is very far superior to the observations by eye and ear.

In 1846 Mr. Airy was one of three Commissioners appointed by the Queen to report on the proper gauge for railways. The Commissioners considered that the chief advantage of the broad gauge lay in the speed of the trains, while in the conveyance of goods and the cost of outlay the narrow gauge was the superior. For these and other reasons they recommended that the present narrow gauge should by statute be that of all railways to be constructed in future. After examining and rejecting several ingenious inventions by which the same carriage could be made to run on both gauges, they also recommended that some equitable means should be found by which the railways then on the broad gauge could be reduced to the narrow. This second recommendation was not, however, adopted by the legislature. In 1858 Professor Airy was one of the Commission on the Ordnance Survey appointed to consider, amongst other questions, the scales on which the maps were to be constructed.

In the 'Monthly Notices' for November, 1846, there is a memoir by the Astronomer Royal, giving his own, Professor Challis', and Mr. Adams' separate accounts of the discovery of Neptune. Mr. Airy remarks that in the whole history of astronomy there is nothing comparable to this discovery; Uranus, Ceres, Pallas, and other planets were discovered by observation, but, in the case of Neptune, mathematicians stated beforehand that a planet would be found exactly in a certain place and presenting exactly a certain appearance. In that

place and with that appearance the planet was found. The predictions of Adams and Leverrier differed by only one degree of longitude. The controversy which has arisen on this discovery cannot be briefly discussed, and must be omitted in a sketch as short as the present one.

In the years 1850, 1851, Mr. Airy turned his attention to antiquarian researches. There are several papers in the 'Athenæum' on the Exodus of the Israelites, and some more on the place of the landing of Cæsar. The first of these lines of enquiry led gradually to the "Notes on the earlier Hebrew Scriptures" (1876), and the latter to the "Treatise on the Roman Invasion of Britain" (1865). Halley, reasoning on the phenomena of the tides as described by Cæsar, and comparing these with the Channel tides as then known, had concluded that Deal was the landing place. Mr. Airy, however, showed that, with fuller knowledge of the local tides, this line of reasoning would prove that Pevensey was the actual landing place. He also contended that this result was confirmed by a study of Cæsar's movements in Gaul before the crossing, and his transactions in the interior of Britain after the passage of the straits. Mr. Airy was also interested in several other antiquarian questions. Thus in the 'Phil. Trans.' for 1853 there is a paper on the eclipses of Agathocles, Thales, and Xerxes, in which he arrived at some new dates for these events. After the publication of Professor Adams' theory of a diminished value of the acceleration of the Moon's mean motion, Mr. Airy repeated his calculations, and somewhat modified his results. He also compared the dates of thirty-six eclipses given in a Chinese historical work called 'Chun Tsew' with those calculated by theory by a French writer, and points out how generally accurate the Chinese records are on these points.

Mr. Airy had the pleasure of viewing three total eclipses of the Sun, the first from the Superga, near Turin, in 1842, the second at Göthenburg, in Sweden, in 1851, and the third at Hereña, in Spain, in 1860. In the many accounts which he has given of these eclipses, he continually dwells on the impressiveness and awfulness of the scene, pointing out that no degree of partial eclipse gave the least idea of a total eclipse. He mentions, on the authority of Arago, that the officers of a French corvette who had been trained to observe the eclipse of 1842 lost their discipline when the darkness came on, and the observations were not made. The most remarkable of all the appearances at the first eclipse were the red mountains or flames seen round the Moon. It was afterwards discovered that these had been seen in 1733 by a Swedish astronomer, but all the observers at Turin were taken by surprise. It was difficult then to decide what they were, or even whether they belonged to the Sun or to the Moon. In the eclipse of 1851 special attention was given to these flames, and,

in his report to the Astronomical Society, Mr. Airy said it was impossible to see the changes in their aspect without feeling the conviction that they belonged to the Sun and not to the Moon. Still many doubted, but in 1860 the observed angular displacements and velocities of displacements of these red appearances as the Moon's disc passed over the Sun proved decisively that they belonged to the latter body. On the expedition to Spain, the Astronomer Royal was accompanied by a large body of observers; some were trained astronomers, the others amateurs. All did good service, there was much work to be done and it was well done. The Government placed the large steamer "Himalaya" at the disposal of the party to carry them to and from their destination, and in conferences on the deck of that ship the different classes of observation were divided amongst those present. All the details of the organisation of the expedition rested in great measure on the Astronomer Royal, and in his report to the Board of Visitors in 1861 he pronounced the enterprise to have been very successful. In his lecture to the Astronomical Society he remarked that it would be advantageous to collect from the various accounts, first, all the facts which relate to one part of the phenomena, secondly, those which relate to another, and so on; and, finally, to arrange these in separate chapters in order that a systematic comparison might be made. In the forty-first volume of the Memoirs of the Astronomical Society, edited by Mr. Ranyard, these comparisons are published, and occupy 792 pages. The mere titles of the chapters are sufficient to show the importance and interest of the work.

At the meeting of the British Association at Manchester in 1860, Mr. Airy delivered a lecture on the solar eclipse of that year to an assembly of perhaps 3000 persons. The writer of this sketch remembers the great Free Trade Hall crowded to excess with an immense audience whose attention and interest, notwithstanding a weak voice, he was able to retain to the very end of the lecture. This lecture he repeated at Cambridge, as the Rede Lecture, in 1864, where it was again well received. It was afterwards translated into Dutch by D. Bierens de Haan about 1877. The charm of Professor Airy's lectures lay in the clearness of his explanations. The subjects also of his lectures were generally those to which his attention had been turned by other causes, so that he had much that was new to tell. His manner was slightly hesitating, and he used frequent repetitions, which, perhaps, were necessary from the newness of the ideas. As the lecturer proceeded, his hearers forgot these imperfections and found their whole attention rivetted to the subject matter. On many occasions Mr. Airy has given successful lectures. In March, 1848, he delivered six lectures at Ipswich on Astronomy, which were first taken down by shorthand writers and, after correc-

tion by their author, published in a collected form. This treatise has been very popular and has gone through several editions. He also lectured in the Town Hall at Neath; in 1851 at the Royal Institution, on the total solar eclipse of that year; and in 1853 on the eclipse of Thales. In 1878 he lectured at Cockermouth on the probable condition of the interior of the earth. Besides these there were many other lectures, some of which will be mentioned further on.

At his Rede Lecture at Cambridge in 1864 Mr. Airy took occasion to point out what appeared to be defects in the system of education as connected with mathematical physics, and he followed up these remarks by a letter to the Vice-Chancellor. To assist in remedying these deficiencies he had already written, in 1861, his now well-known treatise on the theory of Errors of Observation. With the same object he published, in 1866, his "Partial Differential Equations," in which he introduced the novelty of giving stereoscopic views to illustrate the surfaces under consideration. These were followed by a treatise on Sound in 1868. In order to direct the attention of the University to the subject of magnetism, he gave a course of lectures in the Easter term of 1869, at Cambridge. These being attended by crowded audiences, were developed into the treatise on Magnetism which appeared in the following year.

One of the most remarkable of Mr. Airy's investigations is that in which he determined the mean density of the Earth. He had always been much interested in this subject, and in the fourteenth volume of the Memoirs of the Astronomical Society we find that he assisted Mr. Baily in his important repetition of the Cavendish experiment by contributing the theory and the formulæ. In 1826 another method had occurred to him which promised to give a more accurate result than either Maskelyne's method of measuring the attraction of a mountain or the Cavendish experiment. His immediate object was to compare the force of gravity at the surface and at the bottom of a deep mine, using the pendulum as the means of observation. In the 'Phil. Trans.' of 1856 he describes the attempts he had made at Dolcoath in 1826 and in 1828, and their failure on both occasions in consequence of accidents, once by fire and once by water. Twenty-six years passed before he was in a position to repeat the experiment for the third time. In 1854, however, a new power was, he tells us, placed in his hands. The galvanic system had been established at the Royal Observatory, and, in the familiarity which he now possessed with telegraphic applications, he perceived that the difficulty of comparing the upper and lower clocks would be entirely removed. The experiment was conducted at the Harton Pit, a mine about 1260 feet deep. The result was that gravity below was greater than that above by 1/19286th part, so that the mean density of the earth was 2·7 times the surface density and 6·6 times that of water.

The value thus obtained is larger than that given by the Schehallien method, and considerably larger than that deduced by Baily from the torsion rod experiments. After the experiments at Harton Pit were concluded, he gave a lecture at the Central Hall, South Shields, on the pendulum experiments, and, in the next year, a Friday Evening Lecture at the Royal Institution, in London, on the same subject. These experiments were also noticed by "Mr. Punch" in a copy of verses on "Airy and the Coal Hole," written by Shirley Brooks.

In 1867 Professor Airy read a paper at the Institution of Civil Engineers on the use of the suspension bridge with stiffened roadway for railway and other bridges of large span. This paper was the result of a discussion with Mr. R. Stephenson on the method to be adopted for the Britannia bridge, and the author thought there were better methods available for wide crossings than simple tubular bridges. Besides writing this paper, for which he received the medal of the Society, he often attended the meetings and joined in the discussions.

As the management of the expeditions to observe the transits of Venus in 1874 and 1882 would necessarily fall on the Astronomer Royal, Mr. Airy began his preparations as early as 1857. In that year, he called the attention of the Astronomical Society to the means available for correcting the measure of the Sun's distance in the next twenty-five years. On this occasion he pointed out the peculiar advantages of observing Mars for that purpose, showing that at the opposition of 1860 that planet would make a near approach to the Earth. The preparations for the transit were continued during the following years; the proper places to which the expeditions should be sent were discussed at great length and finally chosen. The northern stations having been occupied by Continental observers, the southern ones were, by the advice of the Board of Visitors, divided amongst the English parties. It was also decided that photographic should be combined with eye observations, and an extra grant was obtained from the Treasury for that purpose. By 1873 preparations had so far advanced that an efficient body of observers from all classes, naval, military, and civilian, were collected at the Royal Observatory, and were being instructed and practised in all the practical details of observation with the transit, the altazimuth, the equatoreal, and especially with the working model of the transit. At this time the received measure of the Sun's distance depended on the transits of 1761 and 1769, but mainly on the latter. Though there was a close accordance in the results obtained from the different transits, yet all investigators expressed doubts on their correctness. In the transit of 1761 the results depended almost entirely on an accurate knowledge of the differences of the longitudes of very distant stations. In that of 1769 the result rested in great measure on the observations of a single person, viz., those of Father Hell at

Wardhoe. Another difficulty had also presented itself; it was found that after the planet appeared to have fairly entered upon the Sun's disk it was for some time connected with the Sun's limb by a black ligament. Even after the planet had separated itself from the edge, it did not immediately assume a circular appearance. It is clear that the determination of the precise moment of contact is more difficult than it was expected to be at the time of the invention of this method by Halley. For these reasons the results of the transit of 1874 were looked forward to with the greatest interest. The observations were on the whole successful, though some perplexing difficulties arose. At several places the black ligament was not seen, but a faint thread of light of sensible width, due to the atmosphere of Venus, presented itself. This unexpected appearance introduced an element of uncertainty as to the exact moment of contact. Two hundred and sixteen photographs were taken, but their examination did not realise all that had been hoped for from them. On the return of the several expeditions, the reduction of the observations was proceeded with under the scrutiny of Captain Tupman. The immense labour of these calculations delayed the publication of the complete results until June, 1881, nearly seven years after the event, though a preliminary report was made to the House of Commons in 1877.

In the 'Phil. Trans.' for 1872 there is a curious determination of the magnetic state of the iron in the Britannia and Conway tubular bridges. This investigation was suggested by the peculiar tremor of the iron felt by the hand when a train was passing. Both Sir George Airy and Mr. Stephenson expressed this by saying that the metal seemed to be in a state of "molecular shiver." As all experiments show that iron in this state of tremor is peculiarly subject to the inductive action of external magnetic force, Sir George thought that observations made along the axis of the tube might lead to some important conclusions. One general result was that "in the axis of a rectangular tube, at all points except very near the ends, the action of external magnetic forces in planes normal to the axis is absolutely destroyed." Some strange anomalies were observed in one tube which could not be explained until it was remembered that that tube had had a fall while being raised into its place. Thus the effects of an accident were discovered a quarter of a century after it had occurred by a magnetic experiment.

The system of time signals by which Greenwich time is spread over the country by means of the electric telegraph is in great part due to Sir G. Airy. It should be noticed that the whole system is automatic; the Greenwich clock being once set each morning to the exact time, the signals are distributed without any person having to touch the apparatus.

It is impossible to consider in detail the numerous additions which Mr. Airy has made to our knowledge at various times. It is necessary to pass over with merely a mention such an important memoir as that in which he discusses theoretically the stresses in the interior of a rectangular beam. In another paper he mentions a method of correcting the chromatic dispersion of the atmosphere in observing transits of Venus. He has also written several papers on the comparison of Earth currents and magnetic disturbances, on the diurnal and annual inequalities of terrestrial magnetism, and on some lunar magnetic inequalities. We must hasten on to his last work.

In 1874 the Astronomer Royal brought before the notice of the Astronomical Society a new mode of treating the lunar theory. After giving a rapid survey of the methods hitherto employed, he remarks that the nature of the steps has compelled the investigators to decide the succession of their terms, not by numerical magnitude, but by algebraical order, and that this has produced great inequality of convergence. The mental labour cannot be alleviated by an amanuensis, and he quotes a remark of M. Plana, "Quelquefois ces calculs me font presque perdre la tête." He proposes, as a new method, to begin with Delaunay's final numerical expressions for the longitude, latitude, and parallax with a symbolical term attached to every number for contingent correction. These corrections are so small that it is sufficient to retain only their first power. The expressions are then substituted in the equations of motion with the time for independent variable, and the result of the substitution is a great number of equations for determining the numerical values of a great many small quantities. In this theory the orders of the terms are numerical and equally accurate throughout; the details are so easy, that a great part can be intrusted to a mere computer. Though he was then seventy-three years of age, he had aleady begun the work. He says that, though it is sufficiently possible that he may not be able to complete it, he desires to have it in such a state that a successor may be able to take it up. For this reason in each of the following years he gives further details as to the theory, and describes how far he had advanced in the approximations. Finally, in 1886 the numerical lunar theory was given to the world in the form in which it was left by the author; a wonderful monument of what a man can do at the age of eighty-five. He explains in the preface how the work was delayed by the heavy pressure of business, not only in the ordinary conduct of the Observatory, but also in completing the calculations for the transit of Venus in 1874, and in preparing for that in 1882. He then remarks on some serious discordances which remain to be accounted for; "I cannot conjecture," he adds, "whether I may be able to examine sufficiently into this matter." He never was able.

A long catalogue of the honours and titles which Sir G. Airy

received from the universities and other scientific bodies is given after his name in the List of Fellows of this Society. He was D.C.L. of Oxford, LL.D. of Cambridge and Edinburgh, one of the first members of the Senate in the University of London. He was one of the eight foreign Associates of the Institute of France, and received the Lalande Medal. He was made K.C.B. in 1872. In 1875 he received the freedom of the City of London, enclosed in a gold casket. He was a Fellow of the Royal Society for fifty-five years, received both the Copley and Royal Medals, and was made President in 1871. He was on the Council of the Astronomical Society for more than fifty years, was five times President, and received two gold medals. He was President of the British Association at the meeting at Ipswich in 1851. He was an honorary member of the Institution of Civil Engineers, and received the Telford Medal. He was elected a foreign Associate of the Dutch Academy of Sciences in 1878, and held honorary titles from many Continental and American Societies. He also received the Albert Medal of the Society of Arts, which was presented to him by the Prince of Wales. He was a member of the Royal Society of Edinburgh and of the Royal Irish Academy. He received a diamond snuff-box from the Emperor Nicholas of Russia, and decorations from the Emperor of Brazil. He was Chevalier of the order "Pour le mérite" of Prussia; he belonged to the Legion of Honour of France, and had decorations from Sweden. A gold snuff-box was given to him by the Steam Navigation Company, a silver-gilt inkstand by the River Dee Company, and he held a French Sèvres vase as a Commissioner on Standards.

When Sir George Airy retired from the Observatory at the age of eighty, the Board of Visitors recorded in their Proceedings a resolution expressing, in an emphatic manner, their sense of his eminent services throughout the long period of forty-five years during which he had presided over the Observatory. Among his many services to science they especially mentioned the following:—(*a*) The reorganisation of the Observatory; (*b*) the designing of instruments of exceptional stability and delicacy; (*c*) the extension of the means of making observations on the Moon in such parts of her orbit as are not accessible to the transit circle; (*d*) the investigation of the effects of iron ships on compasses; (*e*) the establishment of time signals. Turning next to his labours in departments of science not directly connected with the Royal Observatory, they called attention to the high estimation in which his contributions to the theory of the tides, to the undulatory theory of light, and to various abstract branches of mathematics are held by men of science throughout the world.

During his residence at Cambridge Sir George married Richarda, the eldest daughter of the Rev. Richard Smith, of Edensor, in Derbyshire. Lady Airy died in 1875, after a long illness. After his

retirement Sir George received a pension from the Government, and resided close to Greenwich Park, and not far from the scene of his former labours. Here, on his ninetieth birthday, he held his last reception. It was attended by a numerous and distinguished company, among whom was one old friend even older than himself.

Sir George owned a cottage at Playford, near Ipswich, to which he often retired for rest and recreation. Here he had spent his boyhood, and here at last he was laid in the grave by the side of his wife. Visiting the village every year, he could remember five generations of more than one family, and could give the early history of most of the others. The last scene of his life may be said to have begun and ended here. On his last visit he had a fall, which in his enfeebled state proved more serious than might have been expected. He never properly rallied, but gradually sank and passed away at his residence in Greenwich. His funeral at Playford Church was quiet and simple, fitting the noble simplicity of his life.

E. J. R.

EDMOND BECQUEREL.—In the long and brilliant roll of physicists of whom France may justly boast, few names deserve a more prominent position than that of Becquerel. The Becquerel family constitutes a true dynasty of *savants*, and affords a twice repeated instance of father and son engaged in the self-same studies, and seated side by side in the same identical section of the Academy of Sciences of Paris.

The second of the family, Edmond Becquerel was born in 1820, and grew up, as remarked in the official *éloge* delivered by M. Duchatre on the occasion of his death, in a scientific atmosphere. From his illustrious father, Antoine César Becquerel, he evidently inherited his acute power of observation, and that "infinite capacity for taking pains," which seems to be the essential characteristic of the Newtons, the Faradays, the Darwins, and, in short, of all the great leaders of science.

While thus born a *savant*, the cause which determined Edmond Becquerel to become a physicist must be sought in the example and the society of his father Antoine, first as a pupil, then as assistant. Under different circumstances he might have become equally eminent as a chemist or as a student of the organic sciences.

As he approached manhood, the discovery of photography burst upon the world, and naturally drew general attention to the chemical action of light. Edmond Becquerel found here his opportunity, and studied with zeal and success the conditions and laws of the novel phenomena. His papers on the chemical radiations accompanying solar and electric light, and on their effects, were of distinguished value. He even succeeded in obtaining a photographic repro-

duction of the spectrum in its natural colours, with the serious drawback that the coloured image could be preserved only in complete darkness. Our advances upon Becquerel in this direction have not been so rapid or decisive as might have been hoped.

Becquerel came to the conclusion that all the effects produced under the influence of light are due to one and the same radiation acting upon different bodies. He considered it probable that in different beings the retina is not always sensitive between the same limits of refrangibility. His principal researches in this direction are to be found in the 'Annales de Chimie' for November, 1848.

It must not be forgotten that Becquerel's view was strongly opposed by Professor Forbes, of King's College, also by R. Crouch, of Polperro, who both maintained that no marine animal has the power of vision under the influence of such rays of light as would not excite the optic nerve of man, and that those depths of the ocean at which an everlasting darkness prevails, are the regions of silence and death.

It need scarcely be said that recent research has fully proved that existing animal species can recognise rays of light which make no impression on the human retina, thus confirming the correctness of Becquerel and refuting his critics.

It is not surprising that with the researches just mentioned he combined a prolonged and exhaustive study of phosphorescence as produced by insolation.

For the better study of these phenomena, he invented his well-known phosphoroscope. By means of this instrument, substances can be viewed immediately after having been exposed to the light of the sun; the interval between insolation and observation being capable of reduction at pleasure and of very accurate measurement. Edmond Becquerel investigated also the light emitted by the phosphorescent compounds of uranium, and turned his attention to the infra-red portion of the spectrum, and to the ultra-violet portion, to which the chemical action involved in the new art of photography was due. He formed a pure spectrum on a sensitive plate of silver prepared by the process of Daguerre, and found that the fixed dark lines of the solar spectrum as recognised by Wollaston, Fraunhofer, and Brewster were traceable in the chemical impression, and that similar lines therefore existed in the parts of the solar spectrum lying beyond the limits of visibility. Becquerel published in the 'Bibliothèque Universelle de Genève' for 1842 a diagram of the fixed lines of the spectrum as enlarged by these researches. These photo-chemical and optical studies, taken conjointly, have been pronounced by Fizeau to be a model of research in experimental physics.

Another research of the highest order was his investigation of the action of magnetism on all substances, especially upon oxygen, the *magnetic* function of which he carefully scrutinised. Here he and

Faraday were working simultaneously almost on the same lines. In a letter to the elder Becquerel, Faraday (1851) speaks in terms of high admiration of Edmond Becquerel's achievements. He was exceedingly struck, he writes, with the beauty of the experiments, with the accuracy of the determinations, and with the results, which confirmed and extended his own.

Edmond Becquerel was profoundly interested in the laws of electro-chemical decomposition, his views agreeing for the most part closely with those of Faraday. What is now known as "Becquerel's Law" is a special application of the general law of Faraday governing electro-chemical decomposition.

Among what may be called Becquerel's minor papers are included researches on the thermic radiation of electric sparks, on the production of phosphorescence by electric sparks, on the determination of high temperatures, on the radiation of solar heat and the production of electric currents, on the electro-chemical decomposition of water, on constant current batteries, on the chemical rays accompanying those of light, on the constitution of the spectrum, on the laws of the evolution of heat during the passage of electric currents through solids and liquids, on coloured rings produced by the deposition of metallic oxides on metals, on the electric conductivity of solids and liquids, on phosphorescence produced by insolation. Altogether, E. Becquerel produced seventy-one memoirs which are mentioned in the Royal Society's index, in addition to a number written in conjunction with his father, Antoine César, and afterwards with his son, Henri.

Having thus briefly summarised his scientific work, we may glance at what must be called the outer events of his life. It is to be remarked that though he had early distinguished himself by scientific works of high value, and as the son of an eminent and much respected Academician he was not without influence, yet none of the great scientific establishments of his country offered him an appointment.

In 1849, under the Second French Republic, the Government of the day created a National Agronomic Institute, and adopted the "eminently liberal principle" of appointing the professors by competition. On this occasion, strange to say, the result was favourable. Becquerel, we are told in his official *éloge*, "submitted himself to this redoubtable test," and, for a wonder, though weighed in a balance so little capable of appreciating his merits, he was fully successful. He thus obtained a chair at this important institution, which had been established at Versailles. Here his lectures were fully appreciated by an audience who followed his teachings with lively interest. His career at the Agronomic Institute was, however, of short duration. In two years political events, which need not concern us, brought on the suppression of the Institute. But as one door was

closed, another and a wider opened. Becquerel was called to a chair at the Conservatoire des Arts et Métiers. Here his eminent talents found ample scope.

The Academy of Sciences meanwhile was eagerly looking forward to receive him to its membership. However, on account of strict limitation of its numbers, this inclusion did not become possible until 1863, when the death of Despretz opened the way.

In 1878 he succeeded his father as Professor of Physics at the Museum, where he was duly admired for profound mastery of his subject, as well as for the admirable clearness of his teachings. This important position he continued to occupy down to his death.

Here he was in a position to contribute powerfully to the advance and the diffusion of his favourite science, by his personal researches which he still carried on with unremitting zeal and interest, and by his teachings at the Conservatoire and the Museum.

Becquerel's health was unbroken, notwithstanding intense application to his duties and his studies. Not merely his personal friends, but the learned world, hoped that, like his father, he might reach an advanced age. Such, however, was not to be his lot. In the beginning of May, 1891, he was seized with an indisposition, which, without apparent cause, rapidly intensified, so that a few days saw the end of his brilliant career.

Edmond Becquerel's scientific services can best be judged from a survey of his researches. It will be seen that he took up physics, especially electricity, from what may be called the chemical side. Some of the present developments of the science would probably appeal to him less strongly.

The personal qualities of M. Edmond Becquerel aided in no common degree the attainment of his high position as a *savant*. He was patient, persevering, and truthful, and he is known to have been what the French expressively call *un homme de cœur*.

It is interesting to note that M. Henri Becquerel has been recommended by the Academy of Sciences as the most suitable candidate to succeed his distinguished father in the chair of Physics applied to Natural History, at the Museum.

W. C.

THOMAS STERRY HUNT was born in Norwich, Conn., on September 5, 1826, of an old New England family. His ancestor, William Hunt, was one of the founders of Concord, Mass., in 1635. His maternal grandfather, Consider Sterry, of Norwich, was a civil engineer and mathematician, and was the author of text-books of arithmetic and algebra, published 100 years ago. Mr. Hunt was destined for the profession of medicine, but, after preliminary studies, his love for chemistry and mineralogy led him, early in 1845, to become a special student, and

afterwards assistant under Professor Benjamin Silliman, sen., in Yale College. Two years later, after working for a short time in the Geological Survey of Vermont, he was selected, on Silliman's recommendation, by Mr., afterwards Sir William, Logan to be the chemist and mineralogist to the then recently-organised Geological Survey of Canada. In this position he remained as the colleague and assistant of Sir William for more than 25 years, till his resignation in 1872. His work in this capacity is well known. He was employed in the earliest scientific investigations of the petroleum, the rock salt, the phosphates, and the iron and copper ores of Canada, and also in researches on the composition of a great number of rocks and minerals, of mineral waters, and of soils; while he devoted a large amount of attention to the structure and composition—at that time so little known—of the ancient crystalline rocks of the Ottawa Valley and the Great Lakes, in unravelling the stratigraphical intricacies of which Logan and his assistant Murray were at the same time actively and most successfully occupied. He thus had an important share in the great work of instituting the Laurentian and Huronian systems of Geology, and in systematising our knowledge of the oldest rocks of Canada and of the world. This work he afterwards followed up independently, in the development of the Norian, Montalban, Taconian, and Kewenian systems, in which he included various groups of ancient rocks between the Laurentian and the Cambrian; and, though some of these groups may be regarded as still in dispute, there can be no question of the great scientific value of Hunt's studies of them, and of the new facts which he contributed to their discussion.

While connected with the Geological Survey, Hunt willingly aided in the drudgery of literary work and administration, for many parts of which his early culture and extensive range of reading and knowledge well fitted him.

At this time also he conceived and published, in a succession of papers, those wide views on chemical and general geology which were embodied in his greater works, and more especially in his 'Mineral Physiology and Physiography' (1886), in which he discussed, with a power and range of knowledge rarely equalled, the original condition of our planet, and the genesis of its more ancient rocks, as well as the processes of decomposition, recomposition, and metamorphosis to which they have been subjected. This great and eminently suggestive work deserves the careful study of all concerned in petrography or physical geography, who, whether or not they may agree with all its conclusions, will find very much to instruct, and to stimulate and guide thought and investigation. This work alone, with the earlier essays on chemical geology, would be sufficient to form the basis of a great reputation, and must retain its place as a leading

authority on the subjects of which it treats. As the author himself states, this work, and more especially the "Crenitic" hypothesis developed in it, are the "result of nearly 30 years' studies, having for their object to reconstruct the theory of the earth on the basis of a solid nucleus, to reconcile the existence of a solid interior with the flexibility of the crust, to find an adequate explanation of the universally contorted attitude of the older crystalline strata, and at the same time to discover the laws which have governed the formation and the changing chemical composition of the stratiform crystalline rocks through successive geologic ages."

To Dr. Hunt we thus owe some of the earliest attempts to subdivide and classify in a scientific manner the stratiform crystalline rocks; a work to which he brought not only his studies throughout Canada and the United States, but also the result of inquiries conducted during repeated visits to the British Islands and to Continental Europe. In pursuing these studies, and while reviewing and controverting various hypotheses, including the igneous or plutonic, the metamorphic and the metasomatic, all of which he rejected as irreconcilable with observed facts, and as violating chemical theory, Dr. Hunt vindicated what he deemed the essential soundness of the still imperfect Wernerian aqueous view, and advanced, as its proper development and completion, his own crenitic hypothesis. According to this theory, the source of the various groups of crystalline rocks was "the superficial portion of a globe, once in a state of igneous fusion, but previously solidified from the centre. This portion, rendered porous by cooling, was permeated by circulating waters, which dissolved and brought to the surface during successive ages, after the manner of modern mineral springs, the elements of the various systems of crystalline rocks. These rocks thus mark progressive and necessary changes in the mineralogical evolution of the earth."

Dr. Hunt never abandoned the scientific pursuit of chemistry and mineralogy. In the former science he summed up the general conclusions of his researches in 1887, in his work entitled 'A New Basis of Chemistry,' which has gone into a third edition, and has been translated into French. His latest work, published in 1891, 'Systematic Mineralogy,' gives a new classification of the mineral kingdom, based on an improvement of what used to be called the Natural History System followed long ago by Möhs and Jameson. It would be premature to express any opinion as to the acceptance by chemists and mineralogists in general of the new views propounded in these works; but they are unquestionably able, and full of important generalisations and suggestions which must make their mark in the science of the future.

Dr. Hunt found time to do some work as an educator. He was

Professor of Chemistry in the Laval University, Quebec, from 1856 to 1862, during which time he delivered annual courses of lectures in French. He continued to be Honorary Professor until his death. He was also for several years Lecturer in M'Gill University, Montreal, and was Professor of Geology at the Massachusetts Institute of Technology, 1872–78. Among his academic titles were those of M.A., Harvard; Sc.D., Laval; LL.D., M'Gill; and finally LL.D., Cambridge, England. He was elected a Fellow of the Royal Society of London in 1859. He was a member of a large number of other societies, both Canadian and foreign. A member of the National Academy of Science of the United States, in 1873, he was President of the American Association for the Advancement of Science and of the American Institute of Mining Engineers, and twice President of the American Chemical Society. He was one of the original members and the third President of the Royal Society of Canada, which, uniting some of the features of the British Association with those of a Royal Society, elects a new President annually. One of the organisers of the International Geological Congress, he was its first Secretary, and was a Vice-President at the Congresses of Paris, 1878, Boulogne, 1881, and London, 1888. In connexion with the great industrial exhibitions, Dr. Hunt represented Canada as a member of the International Juries at Paris in 1855 and 1867, and at the Philadelphia Centennial Exhibition in 1876. He was an officer of the French order of the Legion of Honour and of the Italian order of St. Maurice and St. Lazarus.

In 1878 Dr. Hunt retired from public professional life, and devoted himself mainly to the perfecting of his more important works in new editions, and to the preparation of his 'Systematic Mineralogy.' His health and strength, however, gradually declined, and, continuing to work almost to the last, he passed away peacefully on Friday, February 12, 1892. His death must be deplored as a great loss to science; but he had the good fortune, not granted to all scientific workers, to have means and leisure in his closing years to bring together in a complete and elaborated form all the principal scientific results of the work of his life.

In 1878 Dr. Hunt married the eldest daughter of the late Mr. Justice Gale, a lady of culture and literary taste, who survives him.

J. W. D.

Carl Wilhelm von Nägeli, the son of a country doctor, was born on March 27, 1817, at Kilchberg, a village overlooking the Lake of Zurich. His school days were for the most part spent at the Zurich Gymnasium, and at their close he entered the University of Zurich with the intention of preparing for his father's profession. His inclinations soon led him, however, to pursue scientific and philosophical

studies, rather than medical, and to devote himself more particularly to botany. With this special object in view, he studied for some time under Aug. Pyrame de Candolle at Geneva. He graduated at Zurich in 1840 with a botanical dissertation, the subject of which was the Swiss species of the genus *Cirsium*. Within a short time of his graduation he visited Schleiden, then Professor at Jena, and, coming under the influence of his powerful personality, Nägeli threw himself heart and soul into the microscopic researches which, in the hands of von Mohl and Schleiden, were quickening and developing botanical science. The first fruits of his labours in this direction were his "Beiträge zur Botanik," short papers on various subjects, published in 'Linnæa,' 1842; his important paper "Zur Entwickelungsgeschichte des Pollens" (1842); and his remarkable contributions to the short-lived 'Zeitschrift für wissenschaftliche Botanik' (1844–46).

In 1845 Nägeli married. On his wedding tour he spent some time on the south-west coast of England, where he continued the algological studies begun on a visit to Naples in 1842, the outcome of which was his important work 'Die neuern Algensysteme,' published in 1847. Shortly after this Nägeli, who had for some years been a "Privat-Docent," was promoted to be "Professor extraordinarius" at Zurich. During the next few years he did not publish much beyond a valuable work on fresh-water Algæ ('Gattungen einzelliger Algen') in 1849. He was not by any means idle, however, for he was engaged on various researches, in the prosecution of which he associated with himself, in the year 1854, his pupil Carl Cramer, and which were published in 1855–58, forming the monumental 'Pflanzenphysiologische Untersuchungen.' In the meantime Nägeli had become Professor of Botany at Freiburg-im-Breisgau (1852), and had passed on from there to Munich (1857). Of the work of the ten succeeding years, his fundamental anatomical studies were published in his "Beiträge zur wissenschaftlichen Botanik" (1858–68); the rest he contributed to the 'Sitzungsberichte' of the Bavarian Academy of Sciences, of which he continued to be an active contributing member until late in life. The very numerous papers which he read before the Academy, dealing with a variety of subjects belonging to all departments of botanical science, form three volumes of 'Botanische Mittheilungen' (1861–81). Special mention should be made of his important paper, "Theorie der Gährung," published (1879) in the 'Abhandlungen' of the Bavarian Academy. During this period he also wrote 'Das Mikroskop,' in conjunction with Schwendener (1867, 2nd ed., 1877); and works on the lower Fungi ("Die niederen Pilze in ihren Beziehungen zu den Infectionskrankheiten und der Gesundheitspflege," 1877; also in 'Untersuchungen über niedere Pilze; aus dem Pflanzenphysiologischen Institut in München,' 1882). With the publication of his great work, 'Die

mechanisch-physiologische Theorie der Abstammungslehre,' in 1884, his scientific career may be said to have closed.

Nägeli was ennobled by King Ludwig II in 1878, and was elected a Foreign Member of the Royal Society in 1881. He continued to hold the Botanical Chair at Munich until his death, at the age of seventy-four, on May 11, 1891. His health, already impaired by advancing years and excessive work, had been further weakened by an attack of influenza in 1890, from the effects of which he had never completely recovered.

Most of the foregoing details as to Nägeli's life have been taken from the appreciative notice in 'Nature' of October 16, 1891, by Dr. D. H. Scott, who obtained them from the funeral oration (in 'Neue Zürcher Zeitung,' May 16, 1891) delivered by Professor Cramer, Nägeli's whilom colleague.

With Carl von Nägeli disappears the last member, and not the least distinguished, of that triumvirate of workers who, half a century ago, were so largely instrumental in laying the foundation of the scientific botany of to-day. Of his two coadjutors, Hugo von Mohl, the creator of an intelligible vegetable histology, died in 1872 at the age of sixty-seven; and Matthias Jacob Schleiden—who, though so much of his own work has become obsolete, rendered invaluable service in his relatively short botanical career (1837—1850) by insisting on the study of development as the only basis of a sound morphology, and by inspiring enthusiasm for research in this direction—survived until 1881, having attained the age of seventy-seven.

It is impossible, in the case of a prolific worker like Nägeli, to touch, however briefly, on all or nearly all the important contributions to knowledge which he made. It must suffice to dwell on such of them as may be regarded as epoch-making; but of these there are many, for it has fallen to the lot of but few scientific workers to discover so many fundamental facts as Nägeli did.

If, as appears probable, the "Beiträge zur Botanik" constitute Nägeli's first attempts at research in the field of the new botany, it cannot be maintained that his *début* was altogether brilliant. The reason of this comparative failure seems to be that, inspired probably by personal enthusiasm for his master, he undertook these investigations with the object of supporting the Schleidenian theory of cell-formation, and of defending it against the well-grounded criticisms of von Mohl and of Unger. The actual work is good, even remarkable for the time, but most of the conclusions, and many even of the observations, are vitiated by the only too obvious *parti pris*. It is not until he has regained something of his independence of thought that Nägeli's genius begins to manifest and assert itself. This is clearly observable in the almost contemporaneous paper on the development of the pollen, where he asserts (p. 17) that the

cell-wall is formed not immediately round the nucleus (cytoblast), as Schleiden taught, but round a granular mass in the midst of which the nucleus lies. In this paper also he incidentally corrected the erroneous view that the granules in the contents of the pollen-grain represent spermatozoids, and he observed in certain cases the presence of two nuclei in the grain, though he failed to appreciate the full meaning of the fact. But his comprehension of the nature of the cell was still far below the high level which is marked in his memorable paper "Zellenkerne, Zellenbildung und Zellenwachsthum," which appeared in the 'Zeitschr. für wiss. Botanik.' In that paper his chief objects are the extension of Robert Brown's discovery of the nucleus (1831) to the principal families of Cryptogams, and the investigation of the processes of cell-multiplication. With regard to the former point, he arrives at the wide generalisation that the nucleus is present in all classes and orders of plants, and that its absence from any plant-cell has not been, and probably cannot be, proved; and he also recognises that the nucleus is not a solid mass, but a vesicle, with a proper membrane enclosing fluid granular contents with one or more nucleoli ('Zeitschr. für wiss. Botanik,' Heft 1, 1844, pp. 68 *et seq.*). With regard to the latter point, he gives up the universality of the Schleidenian theory for which he had so stoutly contended, and admits (*loc. cit.*, Heft 3 and 4, 1846, p, 49) that, in vegetative organs at least, cell-multiplication is effected by division, "free cell-formation" being restricted to the reproductive organs.

The chief cause of Nägeli's change of view on the subject of cell-formation was an interesting discovery which, he tells us, he made whilst investigating Algæ at Naples in the summer of 1842. He recognised that the "mucus" in the cell forms a continuous lining to the wall of healthy cells, to which he gave the name "Schleimschicht" (*loc. cit.*, Heft 1, p. 91). It is true that Kützing had, in the previous year ('Linnæa,' 1841), made a similar observation, also in the Algæ, but he had failed to interpret it correctly: he termed it "Amylidzelle," thinking that it was, or could be changed into, starch. Nägeli, on the contrary, ascertained that this layer consists of granular "mucus," which at an earlier stage more or less fills the cell-cavity, and at a later stage forms a parietal layer; and further, that its reactions show it to be nitrogenous. Having discovered this, he felt that the results of his two earlier papers were no longer convincing, since the phenomena which he had regarded as the expression of processes of free cell-formation could be ascribed, with greater probability, to the contraction of the "Schleimschicht" away from the cell-wall. Subsequently (*loc. cit.*, Heft 3 and 4, 1846, p. 52) he carried this discovery to its final stage, asserting that the "Schleimschicht" is never absent from living cells, that it is, in fact, itself living, and that it is from it and by it that the non-nitrogenous cell-wall is formed at its surface.

It is well worthy of note that this subject was at the very same time engaging the attention of von Mohl. The year in which Nägeli announced his discovery of the "Schleimschicht" was also the year in which von Mohl described the "primordial utricle" ('Bot. Zeitg.,' 1844). Similarly, the year in which Nägeli published his more complete account of the physiological importance of the "Schleimschicht" was the year in which von Mohl arrived at the comprehensive conception of this living substance under the name of "protoplasm" ('Bot. Zeitg.,' 1846. See also Nägeli, "Primordialschlauch," in 'Pflanzenphysiologische Untersuchungen,' 1855).

But other treasures lie buried in the now almost forgotten pages of the 'Zeitschrift für wissenschaftliche Botanik.' Heft 1, which was entirely written by Nägeli, contains, besides the first part of the great cell-paper, an interesting account of the remarkable Siphonaceous Alga, *Caulerpa* : a philosophical essay on the fundamental conceptions of Botany; and. finally, a paper with the title "Bewegliche Spiralfaden (Saamenfaden?) an Farren." This paper announces, in fact, the discovery of the antheridia and spermatozoids of Ferns. Although, from the facts of their development, their structure, and their chemical reactions, Nägeli rightly regarded the actively moving, spirally coiled filaments as the equivalents of the spermatozoids of the Mosses and other Cryptogams, still he expressed himself only tentatively on this point, since he had failed to discover the female organ, and had therefore been unable to observe the process of fertilisation. Consequently this discovery remained incomplete until Leszczyc-Suminski detected the female organs on the prothallia of Ferns (1848), and Hofmeister established the homology of these female organs with those of the Mosses, and called them by the same name, "archegonia" ('Bot. Zeitg.,' 1849: 'Vergl. Unters.,' 1851).

In Hefte 3 and 4 of the same periodical, Nägeli published some observations on the Rhizocarps ('Ueber die Fortpflanzung der Rhizocarpeen;" also his criticism of Mettenius, on p. 293) which extended to this group the discovery previously made in the Ferns. He points out that the microspores (at that time known as "pollen-grains") of *Pilularia*, and by analogy those of its allies, do not, as asserted by Schleiden, grow out into pollen-tubes which effect fertilisation as in Phanerogams, but that they give rise to free-swimming spermatozoids. Here also he failed to discover the female organ, borne on the prothallium ("Keimwulst"), developed by the macrospore (then called the "embryo-sac"); and this is the more strange, because he actually saw and figured the neck of the female organ sufficiently clearly to be able to correct Schleiden as to the mode of fertilisation in these plants, pointing out that what Schleiden had taken to be a group of germinating pollen-grains were nothing more than the rows

of cells constituting the neck of the organ which Hofmeister afterwards proved to be an archegonium.

The stage of Nägeli's work has now been reached at which he more especially devoted his energies to the study of the Algæ; and a brief sketch of the results attained in this direction will not be out of place in view of the general biological importance of some of his observations, and of the basis which they afforded for his investigation of other families of plants. The 'Zeitschrift für wiss. Botanik,' in addition to incidental observations on Algæ in some of the other papers, contains four which are exclusively algological: these are "*Caulerpa prolifera*" (Heft 1); "Wachsthumsgeschichte von *Delesseria hypoglossum*" (Heft 2); "*Polysiphonia*" and "*Herposiphonia*" (Hefte 3 and 4). The first of these is of importance in that it gave an insight into the structure, not only of this remarkable plant, but also of the whole family, the Siphoneæ, of which it is a highly-developed representative; it proved that here the whole plant, though attaining a considerable size, and exhibiting unmistakable morphological differentiation into stem, leaf, and root, consists of a continuous mass of protoplasm, unsegmented by cell-walls, and may be termed "unicellular," as Nägeli termed it, though subsequent investigation has shown it to be multinucleate. The paper on *Delesseria*, though of but slight algological interest, is, however, really epoch-making, in that it gives the first account of growth by means of a single apical cell, a discovery which was confirmed by his subsequent observations on *Polysiphonia* and *Herposiphonia* (see also "Wachsthumsgeschichte" of *Pterothamnion* and of *Hypoglossum Leprieurii*, in 'Pflanz. Physiol. Unters.,' Heft 1, 1855). His papers on these two Algæ are of peculiar importance, in that they manifest a clear recognition of the fact that (see especially p. 220, and Plate VII, figs. 1, 2, 3), in the Florideæ at least, the cell-walls are porous, and that the cytoplasms of adjacent cells are connected by protoplasmic filaments passing through the pores of the intervening walls; demonstrating, in fact, that "continuity of protoplasm" which has been rediscovered and studied during the last ten years. The algological interest of these two papers is also great, for they contain some of the earliest observations on the antheridia of these plants, on the tetraspores, and on the cystocarps (as they had been termed by Kützing), which Nägeli considered (see Heft 1, p. 47) to be receptacles for gemmæ, as in *Marchantia*, and consequently termed "Keimbehälter." At this period he does not express any opinion as to the sexuality of the Florideæ; the first definite statement on the subject, unfortunately quite erroneous, occurs in "Die neuern Algensysteme" (1847), where he separates the Florideæ from the rest of the Algæ (including herein the Lichens) on the ground that the latter are destitute of sexual organs, whereas the former possess them in

the form of male organs (antheridia) and female organs, "in which, as a rule, four spores are developed." For several years Nägeli continued to hold the view that the mother-cells of the tetraspores are the female organs of the Florideæ; thus, in his paper, "Beiträge zur Morphologie und Systematik der Ceramiaceæ" ('Sitz.-ber. d. k. B. Akad. d. Wiss.,' 1861; also 'Bot. Mittheil.'), he says, with reference to this view, that up to that time he had seen no sufficient reason for changing it. And yet, in this very paper, he gives an excellent description, with good figures, of the development of the "Keimfrucht" of *Callithamnion*, an organ which, but a few years later ('Mém. de la Soc. des Sci. Nat. de Cherbourg,' vol. 12, 1865; 'Ann. d. Sci. Nat., Botanique,' sér. 5, vol. 7, 1867) Bornet and Thuret proved, by observing the process of fertilisation, to be the true female organ (procarp).

The discovery in the Algæ of the growing-point with an apical cell was probably the inducement which led Nägeli to study the growing-point in other groups of plants. He first described the apical cell and the mode of growth of the stem in various Mosses and Liverworts ('Zeitschr. f. wiss. Bot.,' Heft 2), and subsequently ("Ueber das Wachsthum des Gefässstammes," *loc. cit.*, Heft 3 and 4), he discovered that in *Lycopodium* and in Monocotyledons and Dicotyledons, the growing-point of the stem has no apical cell, but consists of a small-celled merismatic tissue, in which he traced the first indications of the differentiation of the permanent tissues. From this he went on to the fundamental anatomical work which constitutes the main portion of the 'Beiträge zur wiss. Botanik,' in the form of three papers: (1) "Das Wachsthum des Stammes und der Wurzel bei den Gefässpflanzen, und die Anordnung der Gefässstränge im Stengel" (Heft 1, 1858); (2) "Dickenwachsthum des Stengels und Anordnung der Gefässstränge bei den Sapindaceen"; and (3), in conjunction with Leitgeb, "Entstehung und Wachsthum der Würzeln." The last of these papers was for years the authoritative work on the anatomy and growth of the root: it is true that Hofmeister ('Vergl. Unters.', 1851) had already discovered the fact that in many of the Vascular Cryptogams the growth of the root is effected by a single apical cell, but this detracts but little from the value of Nägeli's work. This paper also contains the recognition of the morphological peculiarities of the "rhizophores" of *Selaginella*.

The papers in the 'Pflanzenphysiologische Untersuchungen' which call for special notice are that on the "Primordialschlauch" (Heft 1, 1855), and that on the "Stärkekörner" (Heft 2, 1858). The chief importance of the former is the light which it throws on the physics of the cell, with special reference to the influence of the living 'primordial utricle" on diosmose, a subject which was also engaging the attention of Pringsheim ('Bau und Bildung der Pflanzenzelle,' 1854).

These contemporaneous papers brought into notice those phenomena which, under the comprehensive term "plasmolysis," have come to be of such importance in the explanation of the mechanism of plant-movement. The "Stärkekörner" is a paper which most strikingly illustrates in how remarkable a degree Nägeli combined untiring industry with scientific insight. Taken simply as a descriptive account of the starch-grains, of the variety of their form in different plants, of their structure, and of their chemical composition, it is the most exhaustive that has yet appeared. The preparation of it, which began in 1850, involved, as Nägeli tells us in the preface, the microscopic examination of the roots and rhizomes of about 800 species of plants, and of the seeds of about 1700 species. But the main interest of this paper lies, not so much in the facts, as in the theories concerning the mode of growth and the molecular structure of organised bodies, for which the facts afforded the necessary basis: and further, in the recognition of the importance of the appearance and disappearance of starch-grains as an indication of the metabolic activity of the plant.

In 1846 ('Zeitsch. f. wiss. Bot.,' Hefte 3 and 4, p. 117) Nägeli had expressed the opinion that a starch-grain is a vesicle the wall of which becomes thickened, like that of a cell, by the deposition of successive internal layers. This view, which he had formed, he says, whilst "in Irrthümern der Schule befangen," he now replaces by the well-known theory of "growth by intussusception." This theory, which Nägeli extended also to cell-walls, was generally accepted for many years, until the publication of Strasburger's researches ('Bau und Bildung der Zellhäute,' 1882) when a reaction took place in favour of the theory of growth by apposition. At the present time, however, Strasburger admits ('Histologische Beiträge,' Heft 2) that the growth of cell-walls may be effected by the infiltration of material, a process which, as he says, may be termed "intussusception," but is not altogether the same as that conceived and named by Nägeli, which was inseparably associated with his theory of the intimate structure of organised bodies, generally known as the "micellar theory." According to this theory, organised structures, such as cell-walls and starch-grains, consist of solid ultimate particles which Nägeli at first regarded as being the chemical molecules, but subsequently ('Das Mikroskop,' 1877, p. 354), as groups of molecules termed "micellæ," each of which is, under ordinary circumstances, surrounded by a layer of water. The particles with their watery envelopes are, he argued, held together by the mutual attraction of the solid particles, the attraction of each particle for its watery envelope, and the cohesion of the ultimate chemical molecules of which each particle consists. By acute reasoning based on the study of the physical properties of organised structures, more especially

their "swelling-up" and their behaviour with polarised light, Nägeli arrived at the conclusion that the micellæ are biaxial crystals, and assigned to them as a probable form that of parallelopipedal prisms with rectangular or rhomboid bases. On this theory of their structure, the growth by intussusception of cell-walls and starch-grains would consist essentially in the stretching of the growing layer so as to widely separate the existing micellæ, and in the intercalation of new micellæ into the intervening watery areas.

Brilliant as was this attempt to give a purely physical explanation of certain biological phenomena, and helpful as it undoubtedly was for a time, it must now be confessed that it was inadequate, and that it is to no small extent responsible for the tendency to regard plants more as inanimate objects than as living organisms, which for many subsequent years impeded rather than assisted the advance of knowledge as to the mechanism and physiology of the growth and other movements of plants.

Incomplete as this notice is, and indeed must be, it would be altogether inadequate were it not to include some account of Nägeli's attitude with regard to evolution, a subject upon which he was peculiarly qualified, both by his philosophical habit of mind and his observations as a naturalist, to express a weighty opinion. Throughout his life he was an enthusiastic field-botanist, and acquired a special familiarity with the remarkable phenomena of distribution presented by the flora of his native Alps. With the view of giving precision to his field-observations, he devoted particular attention to the large and highly variable genus *Hieracium*, the thoroughness of his study being attested by a number of papers on the genus in the 'Botanische Mittheilungen,' and by a monograph 'Die Hieracien Mittel-Europas,' prepared in conjunction with Peter, of which the first volume ("Piloselloiden") was published in 1885, whilst the second ("Archieracien") was not completed at the time of his death.

The first definite statement of Nägeli's views on evolution appears in an address delivered before the Bavarian Academy, on March 28, 1865, entitled "Entstehung und Begriff der naturhistorischen Art," which is to a large extent a criticism of the 'Origin of Species.' Beginning, with characteristic completeness, at the origin of living organisms, Nägeli emphasises the importance of Lamarck's assumption that the simplest organisms were, and are continually, formed by spontaneous generation. Going to the consideration of Darwin's theory of evolution by natural selection, Nägeli regards it as being based essentially on the principle of utility, "die Nützlichkeitstheorie ist der Darwinismus"—a principle which he considers to be quite inadequate to explain the facts of the phylogeny of either plants or animals. Darwin's theory, which involves the assumption that variation takes place indeterminately in all directions, both higher

and lower, requires, so Nägeli thought, to be modified and supplemented by the "Vervollkommnungstheorie," inherited from Lamarck, according to which variation takes place determinately and in the higher direction only. From this point of view, Nägeli emphatically asserted that the degree of variability which gives rise to more or less constant varieties or races is the result, not of external, but of internal, causes, an assertion which has since been strongly supported, though on different grounds, by Weismann and others.

The whole matter is fully discussed in his 'Mechanisch-physiologische Theorie der Abstammungslehre,' published nearly twenty years later, where, though there is greater elaboration, there is no essential change of view. In this work Nägeli introduces the idea of a material basis for heredity in the form of what he terms "idioplasm." The characters which an organism inherits from its parent or parents are transmitted by the idioplasm of the reproductive cell or cells, in which all the properties of the adult form are inherent. But the idioplasm of any one generation is not identical with that of either its predecessor or its successor. It is always increasing in complexity as regards both the arrangement and the constitution of its micellæ, with the result that each generation marks an advance, though not always an immediately appreciable one. But the variation which is primarily due to the inherent properties of the idioplasm is not altogether unaffected by external conditions; when these act continuously in a given direction they may cause modifications which are, however, merely adaptive. In a word, whereas, on the Darwinian theory, all organisation is essentially adaptive, according to Nägeli the development of higher organisation and of more complete division of labour is the visible expression of the spontaneous evolution of the idioplasm.

Although Nägeli's theory of evolution, as laid down in the 'Abstammungslehre,' has not met with general acceptance, more particularly as regards the small importance which he attaches to natural selection, still the work as a whole cannot be judged as other than a worthy close to the labours of its author. Full as it is of the accumulated knowledge and experience of a long and earnest scientific career, it constitutes one of the most valuable and suggestive recent contributions to the theory of reproduction and descent.

S. H. V.

PHILIP HERBERT CARPENTER, the fourth son of Dr. W. B. Carpenter, was born at Westminster, on February 6, 1852. He received his earlier education at University College School; and in 1871 commenced residence at Cambridge as a scholar of Trinity College. He graduated B.A. in 1874, in the First Class of the Natural Sciences Tripos, and proceeded to the further degrees of M.A. in 1878, and D.Sc. in 1884.

After graduation he spent some time in the University of Würzburg, working under Professor Semper; and in 1877 he was appointed Assistant Master at Eton College, in special charge of the teaching in biology, a post which he held up to the time of his death, on October 22, 1891.

Herbert Carpenter was a naturalist from the first, and took part in some of the most important biological movements of recent times. He accompanied the earlier dredging expeditions which preceded and rendered possible the famous cruise of H.M.S. "Challenger;" he was a prominent member, and one of the earliest, of the new school of biology at Cambridge; and his appointment on the staff of one of the greatest of public schools was hailed as a significant recognition of the educational value of biological science, and of the importance of making adequate provision for it in schools.

When only sixteen years of age he accompanied his father on the deep-sea exploring expedition of H.M.S. "Lightning." In the following years he took part in the dredging cruises of the "Porcupine," and in 1875 he was one of the scientific staff on board H.M.S. "Valorous," which accompanied Sir George Nares' Arctic Expedition as far as Disco Island, for the purpose of sounding and dredging in Davis Strait and in the North Atlantic.

On these expeditions he was mainly occupied in chemical and physical observations, and it was not until he went to Würzburg, in 1875, that he commenced the special study of the group of animals, the Echinodermata, to which the remainder of his life was so successfully devoted. The choice of this group was almost an accidental one, and was determined in the first instance by the opportunity his residence at Würzburg gave him of examining Professor Semper's specimens, and so of determining the real extent and cause of the discrepancies between the results obtained by his father and by Professor Semper, with regard to certain points in the anatomy of the recent Crinoids.

Professor Semper was much interested in Carpenter's work, and on the completion of his first paper, placed in his hands the important collection of Actinometræ he had himself obtained from the Philippine Islands. The examination of this material occupied Carpenter for nearly two years, and led to the publication of an elaborate and very important monograph on the genus, in the Transactions of the Linnean Society, which established his reputation as an authority on the group. On the return of the "Challenger" Expedition, and the distribution of the collections, the free-swimming Crinoids were at once entrusted to Carpenter, and on the death of Sir Wyville Thomson, in 1882, the stalked Crinoids were handed to him as well. Carpenter's reports on these two groups, published in 1884 and 1888 respectively, are of a most elaborate and complete

character, and rank among the most valuable contributions ever made to our knowledge of the group.

During the preparation of the '"Challenger" Reports,' and after their completion, Carpenter was continuously engaged in the study both of Crinoids and of other groups of Echinodermata, and the long list of his published papers bears striking evidence to his industry and enthusiasm. He did not himself work much at the embryology of the group, but he paid very special attention to the fossil members: indeed he stands out prominently as one of a comparatively limited number of zoologists who have given practical and emphatic recognition to the fact that zoology and palæontology must not be taught or thought of as separate sciences, or be worked at by separate investigators, but must be considered and treated as one great branch of science. "I have," he says, "the strongest conviction (and many mistakes would be avoided were it a universal one), that the only way to understand fossils properly is to gain a thorough knowledge of the morphology of their living representatives. These, on the other hand, seem to me incompletely known if no account is taken of the life-forms which have preceded them." It is to this thorough-going recognition that fossils are not merely parts of animals, but parts of animals akin to those now living on the earth, that the special value of Carpenter's palæontological work is due. Perhaps his most important contribution from this point of view is the admirable catalogue of the Blastoidea in the British Museum, of which he is joint author with Mr. Robert Etheridge, jun.: many of his smaller papers on palæontological subjects are also of great value.

As a zoologist, Carpenter was admittedly the leading authority on the group he had made so specially his own. He was enthusiastically devoted to his subject, and always ready to show his specimens and discuss his results with any brother naturalist. His own work is characterised by extreme thoroughness and conscientiousness rather than by brilliancy, and is perhaps for this reason more certain to endure. As a teacher he never had more than imperfect opportunities; but, working under circumstances in many ways discouraging, he achieved very considerable success; and amongst his pupils are some who have gained marked distinction in the science they first learned to know and to respect through him.

He was a kindly, generous, and unassuming man, whose untimely death will be mourned by friends in many nations.

A. M. M.

SIR WILLIAM CAVENDISH, seventh DUKE OF DEVONSHIRE, Knight of the Garter, was born on April 27, 1808. His father was Mr. William Cavendish, who married Louisa, daughter of the first Lord Lismore. His grandfather, Lord George Augustus Cavendish, third son of the

fourth Duke of Devonshire, was created Earl of Burlington in 1831. The Cavendishes have been conspicuous in English history for several centuries, but here it is only necessary to note the late Duke's connexion with two of the most eminent philosophers this country has produced. His great-grandmother, wife of the fourth Duke of Devonshire, was the Lady Charlotte Boyle, daughter of the Earl of Burlington and Cork, and the direct descendant of Richard, second Earl of Cork, who was brother to the Hon. Robert Boyle, the celebrated chemist and natural philosopher of the 17th century. His great-grandfather was first cousin to Henry Cavendish, the no less celebrated chemist and natural philosopher of the 18th century. The late Duke was educated at Eton, and at Trinity College, Cambridge, and his career at the University was exceptionally brilliant. In the Mathematical Tripos of 1829 his name appeared as Second Wrangler, Philpott, lately Bishop of Worcester, who survived him only a few days, being the Senior Wrangler. At the ensuing examination for the Smith's Prizes, the order of their names was reversed, and they both appeared in the First Class of the Classical Tripos. In the same year he married Lady Blanche Howard, daughter of the Earl of Carlisle; and in the following year he was returned as colleague of Lord Palmerston, to represent the University in Parliament. It might have been expected that the descendant of the staunch friend of Lord William Russell would side with the Whigs; he was not, however, a man to be guided merely by tradition, or to take his opinions at second hand, but was Liberal by conviction, and had the courage of his opinions. In the debates on the Reform Bill, which ensued almost immediately after his return to Parliament, he spoke in favour of the measure, and in a few thoughtful and wise words pointed out the dependence of good government upon the confidence and support of the people. This was too much for his academic constituency, and at the next election he and his colleague lost their seats. He continued, however, in the House of Commons for a year or two, representing first Malton, and then Derbyshire, until in 1834 he succeeded his grandfather as Earl of Burlington.

In the Upper House he very rarely spoke. It cannot be said that he took little interest in politics, for he was a keen observer of the course of events, and formed a shrewd judgment of their issues; but he never laid himself out for debating. He was too conscientious, too cautious of using idle words, ever to be a ready speaker. The condition of Ireland, where the just claims of his tenants were never forgotten by him, gave him great concern; and he was a consistent supporter of all measures for bettering the condition of the people, and removing the grievances which alienated them from England. Yet he dissented on principle from Mr. Gladstone's method of dealing

with Home Rule in Ireland, and became the chairman of the Loyal and Patriotic Union.

Although so little prominent in politics, he never shrank from coming forward whenever his influence, his counsel, or his wealth could be used to advance the interests of the community. In the extension of education, among all classes, and in all aspects, he was, perhaps, best able, as well as most willing, to take the lead. He was Chancellor of the University of London, from 1834 to 1856; and, on the death of the Prince Consort, in 1861, was elected without opposition Chancellor of the University of Cambridge. This office he filled until his death. That University, in 1861, was beginning to enlarge its bounds and widen the range of its teaching, and he not only watched its progress with never-failing sympathy, but contributed to it very materially by his noble foundation of the Cavendish Laboratory, over which Clerk-Maxwell presided. As Chancellor he had sometimes difficult questions to solve, sometimes personal matters requiring care and tact to decide; yet the interests of the University never suffered in his hands, and his decisions were invariably accepted by all parties. He looked, however, far beyond the limited class who can graduate at Cambridge, and when the movement was started for carrying University teaching into the large towns, he cordially supported it.

He was President at one time of the Owens College, at Manchester, and a generous benefactor both of it and of the Yorkshire College, at Leeds. After the foundation of the Victoria University, he became its first Chancellor. The importance of science in relation to the industrial progress of the nation he highly appreciated, and the report of the Royal Commission on Technical and Scientific Instruction, over which he ably presided, has contributed in no small degree to the awakening of public attention to that subject. On the formation of the Iron and Steel Institute, he became its first President. To agriculture he was always devoted. He was one of the founders of the Royal Agricultural Society, and in 1869 its President; and with his accustomed generosity he contributed to the foundation of the Agricultural College at Cirencester. Quite recently, at the instance of the Minister for Agriculture, he moved the University of Cambridge to take steps for promoting the study of scientific agriculture.

He managed his own estates, and they could hardly have been better managed, for he always thought of his tenants' interests. When, some thirty years ago, agricultural rents were everywhere rising, he made no changes in his agreements, and the result was that when the depression came he lost neither tenants nor rents. But he was never content merely to leave things as he found them. For him it was a duty to the country to develop its resources and

improve the condition of the people. A large part of the valuable hematite iron ore of the Furness district lay on his estate, and Barrow, which in thirty years has grown from a small village to a town of over 50,000 inhabitants, with its docks and its ironworks, owes its existence and its prosperity in great measure to his sagacity in enterprise, and to the liberality and earnestness with which he carried out his plans. In a different way, but in the same spirit, he promoted the building of Eastbourne.

His own life was of the simplest. Most of it was spent at Holker, near Grange, in Lancashire. There he took part in the ordinary business of the county and neighbourhood, and for fifty years was chairman of the Board of Guardians for the Poor. He lost his wife in 1840, but the education of his children was his personal concern. To train his sons to take their place in the State, and to watch their careers, was his especial delight. It will be understood how acute must have been to him the suffering when his second son, Lord Frederick Cavendish, Chief Secretary for Ireland, was murdered in the Phœnix Park. His third son, Lord Edward Cavendish, also predeceased him, but only by some months. He passed away peacefully and painlessly, in his eighty-fourth year, on December 21, 1891. Rank, wealth, intellectual gifts, had no power to affect the simplicity of his character, or lessen the deep sense of duty which controlled all his actions.

G. D. L.

James Risdon Bennett was the son of a learned dissenting minister, the Rev. James Bennett, who preached for many years at Falcon Square, in the City. His mother was a descendant of Risdon Daricott, the Evangelist of Somerset in the middle of the eighteenth century.

He was educated first at Sheffield and afterwards at the University of Edinburgh, where he took his degree of Doctor of Medicine in 1833, in his twenty-fourth year. After this he travelled for a time on the Continent, and had some thoughts of settling for practice in Rome. He returned, however, to London, and, after a short connexion with Charing Cross Hospital, was appointed Assistant Physician to St. Thomas's Hospital, which was then still occupying the site of the old monastery and sick refuge of St. Thomas in the street in Southwark which still retains that name. Here, Dr. Bennett afterwards became full Physician and Lecturer on Medicine. He had previously joined the hospital at Victoria Park for diseases of the chest, which had been founded in 1848.

He was much interested in the physical diagnosis of affections of the heart and lungs, and was proficient in stethoscopy, when this branch of clinical medicine was still little known and even slighted as a French innovation.

Meantime, Dr. Bennett had married and taken a house in Finsbury Square, where he obtained considerable family and consulting practice, and formed the friendship of Dr. Jeafferson, Dr. Gull, Dr. Peacock, and Dr. Herbert Davies, who were then his neighbours.

In 1875 he was elected a Fellow of the Royal Society, and in 1876, after serving several offices in the College of Physicians, he was chosen to fill the highest official position in his profession by being made its President. In the presidental chair, Dr. Bennett was at his best. Courteous and dignified, patient and intelligent, never pompous and never weak, he showed on every occasion his good judgment, good temper, and good sense.

On his retirement, after five years service, he received the well-earned honour of knighthood, and always retained the esteem and confidence of the College. He had no pretention to oratory, but his clear-headed and pithy remarks were always listened to with attention. At the Council of the College and on the Council of the Royal Society he was valued for his friendliness, his sagacity, and his power of silence as well as of speech.

Sir Risdon Bennett took an active part in the preparations for receiving the International Congress of Medicine which met in London in 1881; and, as Chairman of the Reception Committee, welcomed the visitors in a French speech which was delivered with remarkable vigour, and was understood as well as applauded by all present, including the Frenchmen.

He had moved to Cavendish Square in the year 1876, and died there of gradual but rapid senile decay at the ripe age of eighty-two.

He was a man of tall and dignified presence, and pleasant, unaffected manner. Strictly upright and honourable in his professional and private life, he was fond of conversation and society, a good talker and a good listener, with no trace of envy in his composition, and a cordial recognition of the merits of his compeers. Otherwise happy in his family, he sustained a heavy loss in the death of his eldest son, who, after a promising career at Cambridge, had only lately been called to the Bar. With this exception, Sir Risdon Bennett's course was one of uninterrupted prosperity. His piety was deep and unobtrusive, and led him to devote much time and labour to philanthropic work. He was well read in general as well as medical literature, but published little—an essay on Hydrocephalus, written when he was a young man, the Lumleian lectures on thoracic tumours, and a few cases in the medical journals—one published only a year before his death. Perhaps his most characteristic appearance in print was a paper advocating counter-irritation as a mode of treatment, which appeared in the 'Practitioner.'

His high character, his sagacious judgment, and the unaffected sincerity of his convictions secured for him general respect and esteem,

and he deserved the praise due to those who pursue a liberal profession in a liberal spirit and who form a link between practice and science.

P. H. P. S.

Sir Edward Sabine was the fifth son and ninth child of Mr. Joseph Sabine, of Tewin, Herts. The Sabines were originally of Norman extraction, and were settled in Kent, at Patricksbourne, until the beginning of the eighteenth century, when Joseph Sabine migrated to Kilmolin, co. Wicklow; several of his descendants are buried at Powerscourt, in the same county. At one time there was a baronetcy in the family, for John Sabine, of Eyre, in the parish of Gravenhurst, in Bedfordshire, was created a baronet March 2, 1670. He left no male issue, hence the title became extinct. At an earlier period, in 1649, one of the name, an Alderman Sabine, left a sum of money for charitable purposes to the city of Canterbury.

Sir Edward's great grandfather, Joseph Sabine, had served with great distinction in Marlborough's campaigns, and was rewarded with the Governorship of Gibraltar, where he died in 1739. He purchased in 1715 the property of Tewin (sold in 1810). A great uncle was killed at the battle of Fontenoy.

Sir Edward was born in Great Britain Street, Dublin, October 14, 1788, and his mother, Sarah, daughter of Rowland Hunt, Esq., of Boreatton Park, Salop, died within a month of his birth. The child was taken by his father and sisters to the care of a Mrs. Davies, of Bath, a warm friend of his mother's.

Of his brothers, two made their mark in life. The second, John, was a captain in the 25th Foot, and apparently a good accountant, for he was complimented by the Horse Guards in 1800 for the way in which he had managed the accounts of the regiment. The eldest brother, Joseph, who died in 1837, was a distinguished Fellow of the Society. He was the first secretary to the Horticultural Society, as we learn from the notice of his works, 'Abstracts of the Papers,' vol. iv, p. 15.

At the age of 14, Edward Sabine went to school at Marlow, and being the quickest among all the boys he was called up by the mathematical masters to be examined in order to form the course of instruction at the school.

In January, 1803, he went to Woolwich, where he remained only one month in each class, and obtained his first commission at the early age of fifteen years and two months, in December, 1803, after a little over ten months as a cadet. So urgent was the occasion that the professors and masters were called on to forego their usual vacation to forward the public service.

His subsequent commissions were dated as follows:—Captain,

1813; Major, 1837; Lieutenant-Colonel, 1841; Colonel, 1851; Major-General, 1856; Lieutenant-General, 1865; and General, 1870. He was gazetted K.C.B. in 1869.

After a year at Woolwich he proceeded to Gibraltar, and returning in 1807 was appointed to the Horse Artillery, in which he served at various home stations until the end of 1812. On his promotion in January, 1813, he fell to a company in Canada, and it was on his voyage to Halifax in the "Manchester," Falmouth packet, that he had the first opportunity of showing how gallant a spirit was to be afterwards diverted from the pursuit of military distinction into other channels. The "Manchester," when eight days out, was attacked by one of those American privateers which swarmed at that period to such an extent that they captured one out of every four of the Falmouth packets in 1812. After a running fight of twenty hours' and a close engagement of over an hour, she had to strike her colours to greatly superior force, on the 24th June, in latitude 47° 10′ N., longitude 24° 10′ W. The privateer was the "Yorktown," Captain Rider, a cut-down East Indiaman of 500 tons, carrying nine long 12-prs. and a crew of 116 men, of whom thirty-six served as marines. The "Manchester" had only three 9-pr. carronades, two long and two light 6-prs., with a crew of only thirty-six men, including passengers, and only two could be spared to return the musketry fire of the enemy. That an obstinate resistance was offered with such disparity of strength, that the British packet fought until she had only 10 lbs. of powder left, and the 9-prs. were reduced to firing 6-pr. case-shot, those guns being disabled, was pre-eminently due to the gallantry of Captain Sabine seconding her commander, Captain Elphinstone. "Captain Sabine and his servant" (a gunner of the old school) "were of the greatest assistance to me," wrote the commander, "and the enemy has confessed that my stern chasers, which were pointed by Sabine and a Mr. Bell, passenger, so completely annoyed them, and did them so much damage, that, although greatly our superiors in sailing, they were loth to range alongside." Sabine himself wrote a detailed account of the action, in which he observed: "We had the satisfaction to find, notwithstanding the difference of metal, the 'Yorktown' had received as much material injury as the 'Manchester,' her foremast and foretop-gallant-mast being shot through, and her hull much damaged; the 'Manchester's' mainmast and maintop-mast were struck in several places, and scarcely a rope left whole in the ship, the grape-shot and musketry having been very heavy, especially at the close of the action when we neared one another. The enemy fired high, and our bulwarks were very good, which accounts for only three men being wounded—two with grape and one by a musket-shot. The 'Yorktown's' bulwarks were 14 inches solid timber, which formed a

protection for their men which it was scarcely possible for our shot to penetrate. We knew, however, that one man was wounded, by the doctor's acknowledgment, and had reason to suppose that there were more, from an observation of the lieutenant: 'That three or four young hands had fancied themselves hurt.' We were repeatedly told both by the officers and crew of the privateer that they wished we had never fallen in with them, both in consequence of the expenditure of powder and shot, and of the injury done to their foremast. We fired above 400 rounds. They must have fired many more." He mentions that every gun of the enemy was loaded with a *double-headed* shot and a bag containing sixty grape-shot.

The Americans behaved very well. Sabine commended their discipline. They respected private property, and treated their prisoners with every respect. The master, mate, and crew were transferred a couple of days later to another prize. The captain and passengers remained on board in charge of a prize-master, and with a crew of seven men set sail for New York. As good fortune would have, they fell in with the "Maidstone" frigate on Sunday, 18th July, and were recaptured; the privateer herself having been taken by the "Maidstone" on the previous day. Thus this little episode came to an end. Instead of being landed prisoners at New York, Sabine and his companions were set on shore at Halifax, his port of destination, and after a week's delay he was sent round to Quebec.

It was during his service at Quebec in the winter of 1813–14, that an incident occurred,* which Sabine was fond of quoting in later life, in illustration of the value of sometimes thinking for one's self. Among the little frontier operations of 1814 was an advance of some American militia on Quebec; and he was sent with a small party to garrison an outpost. He was directed to take a 4-pr. gun with him; but finding in the arsenal a light 24-pr. howitzer, which had been left there by General Burgoyne when he went on the Saratoga Expedition, he thought he could manage to transport that; and with or without permission he did so. He found some coehorn shells, and took those, and a supply of fuzes. He reached his blockhouse before the Americans, under a Colonel Williamson, appeared: who, arriving in the evening, were settling in their camp and busy about cooking, when, to their great astonishment, one of these extemporised shrapnels was fired at them with considerable effect. They forthwith retreated to, what they thought, a safe distance; but another shell burst among them, and seemed to throw them into great confusion. They still further retreated, until fairly out of range. In the morning, when Colonel Williamson ordered them to advance to attack the post, a man stepped forward and declared he would

* This incident the writer learned from the late Rev. T. R. Robinson, D.D., F.R.S.

do no such thing, but intended to return home immediately. He was ready to fight to the death in defence of his own country; but he did not see the sense of invading that of others when they had enough of their own. The commander ordered him to be arrested; but, to his dismay, no one obeyed the order, and when he threatened them they said they had all agreed with the spokesman, and would go home—and so they did.*

Captain Sabine's next service was on the Niagara frontier, where he was favourably mentioned in despatches; and was long the last survivor of the battery, and was privileged to wear the word "Niagara" on dress and appointments.

He was in command of the batteries at the siege of Fort Erie in 1814.

In the year 1816 he returned to England, and then he first began to direct his attention to terrestrial magnetism and pendulum observations, at the house of his brother-in-law, Mr. Henry Browne, F.R.S., 2, Portland Place, to whom he was indebted for first directing his thoughts to these sciences. This house was more or less his home for many years; there he met frequently Captain Henry Kater, F.R.S., and became attracted forcibly to similar lines of inquiry; and thence he started on his successive expeditions. He had, however, doubtless studied practical astronomy previously, for in 1818 his reputation as a skilful observer was such that the President and Council of the Royal Society recommended him for the appointment of Astronomer to accompany the Expedition, sent in search of a north-west passage, under Commander (subsequently Sir) John Ross in that year.

His attention, however, was not solely directed to physics, for his report on the biological results of the expedition appeared in vol. 12 of the 'Transactions of the Linnean Society,' and it embraced twenty-four species of birds of Greenland, of which four were new to the list, and one, the *Larus Sabini*, described by his brother Joseph, entirely new.

On his return Sabine was not long allowed to be idle, for on the equipment of a second expedition in 1819, under Lieutenant-Commander (subsequently Sir Edward) Parry, he was again selected to accompany it.

Parry, in the introduction to his Journal, published 1821, acknowledges Sabine's labours in these terms:—

"The various observations made on board the 'Hecla' during the voyage, have been carefully collected into tables, on the model of those of Wales and Bayly, by Captain Sabine, to whom I am indebted for the arrangement of nearly the whole of the Appendix, and for the

* Captain Sabine learned these details afterwards in New York from some of the officers present.

superintendence of that part of the work during its progress through the press. I feel it no less a duty than a pleasure to acknowledge that in the performance of this task, Captain Sabine has added another to the many obligations I owe him for his valuable advice and assistance during the whole course of his voyage, to the credit of which his individual labours have so essentially contributed."

It might have been added that he contributed not a little to the cheerfulness and harmony of the expedition, by consenting to edit the 'North Georgia Gazette and Winter Chronicle,' a weekly publication established during its tedious stay at Winter Harbour, where the sun was ninety-six days below the horizon. It extended to twenty-one issues. The total strength of the expedition being but eighteen, the editor had to rely very much on his own pen. The result was so highly appreciated as to call for republication.

Sabine did not accompany Parry on his second voyage, the post of astronomer being filled by the chaplain, the Rev. George Fisher. He was himself selected to conduct a series of experiments for determining the variation in the length of the pendulum vibrating seconds, in different latitudes, a subject which had engaged his attention in the first voyage.

Sabine in the pursuit of this investigation visited St. Thomas (Gulf of Guinea), Maranham, Ascension, Sierra Leone, Trinidad, Bahia, Jamaica, in 1821–22; New York, Trondhjem, Hammerfest, Greenland and Spitzbergen in 1823. No less than five men of the Royal Marines, who had been placed at his disposal by Captain Clavering, R.N., as assistants at Sierra Leone, were carried off by fever during a stay of about five weeks in that pestilential climate.

Sabine's observations of the "magnetic" inclination and force at St. Thomas in 1822 were probably the first ever made on that island. Utilised as a base of comparison with the recent magnetic observations of the Portuguese, they become important in showing the remarkable secular change that has been in progress in those elements during the interval.

The remarkable account of his pendulum experiments, printed in a 4to. volume by the Board of Longitude in 1825, must always remain an enduring monument to his scientific merit. It would be impossible in this brief notice to give any adequate idea of the indefatigable industry, the spirit of inquiry, or the range of observation evinced. The work was honored by the Lalande Gold Medal of the Institute of France, in 1826.

The subject of pendulum experiments was one in which he long took great interest; in the years 1827 to 1830 he made experiments to determine the relative lengths of the seconds pendulum in Paris, in London, in the Royal Observatory at Greenwich, and at Altona, and he afterwards determined the absolute length at Greenwich.

Finally, in 1864, he moved the Government of India to undertake the series of pendulum observations at various stations of the Great Trigonometrical Survey, from the sea level at Cape Comorin to the lofty tablelands of the Himalayas, which have thrown so much light on the constitution of the earth's crust, and on local variations of gravity.

The year 1826 was marked by his happy union with Miss Elizabeth Juliana Leeves, daughter of William Leeves, of Tortington, thenceforth his inseparable companion, his invaluable and devoted assistant, and latterly his vigilant guardian, until their union was dissolved by her death, in 1879, after fifty-three years of married life. Her mastery of the German language was something exceptional. She made herself competent to share in every investigation her husband undertook, and habitually examined and checked his work; suffice it to say that thenceforward his powers were doubled.

Captain Sabine's next service, after the publication of his pendulum experiments, was as a Joint Commissioner, in 1825, with Sir John Herschel, to take part with a Commission, nominated by the French Government, to determine the precise difference of longitude between the observatories of Paris and Greenwich, by means of rocket signals. Herschel and M. Largeteau were the observers on the French side of the Channel at Lignières, Captain Sabine and Colonel Bonne on the British side, at Fairlight Downs, near Hastings. The difference of longitude thus found was 9 m. 21·6 s., which was believed to be not more than one-tenth of a second in error, and extremely unlikely to prove erroneous to twice that amount. The accepted difference at the present day of electrical signalling is 9 m. 21·0 s., a slightly larger error, but the determination was very close. In 1827, he compared the length of the seconds pendulum and the magnetic force of the earth at the same two stations.

Having obtained from the Duke of Wellington, then Master-General of the Ordnance, a general leave of absence from military duties, as long as he could usefully be employed in scientific pursuits; he became in 1827 one of the secretaries of this Society, to which he had been elected a Fellow in 1818. This office he filled down to 1829.

The condition of Ireland in 1830 was, as it has been usually since, one to occasion the gravest anxiety. There was a failure of the potato, from the effects of a cold wet summer; local famines; an epidemic of influenza; constant collisions between the peasantry and the police. Under such circumstances Captain Sabine was ordered to join his company: he served with it, or occasionally on the Staff of his friend General Sir James Douglas, K.C.B., for the ensuing seven years, and acquired the character of being a very "smart officer:" but the time was by no means lost to science. He published

two Pendulum papers in 1830 and one in 1831. In 1834 he commenced, in conjunction with the Rev. Humphry Lloyd (afterwards Provost of Trinity College), and Captain James Clarke Ross, R.N. (afterwards Sir James Ross, of Antarctic fame), the first systematic magnetic survey ever made of the British Islands. He extended it single-handed, to Scotland, in 1836, and to England, in conjunction with the same and some additional observers, in the following year. With the exception of the mathematical section of the Irish Report, which was Professor Lloyd's, the Reports—which were published by the British Association—were mainly his; and also a very large share of the observations, more particularly the laborious task of combining them, by equations of condition, to obtain the most probable mean results. About twenty-three years later (1858-61), with unabated interest, and still privileged to number Dr. Lloyd among his coadjutors, he undertook, at the request of the General Committee of the British Association, to repeat this Survey; and, as before, reduced and reported the results, as far as concerned the elements of Dip and Force. Captain (afterwards Sir F.) Evans, R.N., Hydrographer to the Admiralty, dealt with the Declination.

The year 1836 will ever be memorable in the history of British Science, for a letter, dated 22nd April, addressed to H.R.H. the Duke of Sussex, President of the Royal Society, by Baron Alexander von Humboldt, in which he referred to conversations he had held with Sabine and Lloyd, on a recent visit which they had paid to Berlin, and he urged upon the British Government to establish regular magnetical stations in different parts of the British Empire, similar to those which, mainly through his influence and exertions, had already been some years in operation in Northern Asia. This letter was referred to the Astronomer Royal, Mr. (afterwards Sir George) Airy, and Samuel Hunter Christie for a report.—'Roy. Soc. Proc.,' vol. 3.

A Committee on Mathematics and Physics was appointed in May, 1838, of which Major Sabine and Professor Lloyd were prominent members, and towards the end of the year the definitive and official recommendation was made to H.M. Government, already prepared to receive it, to establish magnetic observatories at selected stations in both hemispheres, and despatch a Naval Expedition to the Southern Hemisphere, with the purpose of making a magnetic survey of the Antarctic regions.

It is needless to say that Major Sabine played an active and conspicuous part in all these negociations and preparatory arrangements.

The observations were placed under the charge of Lieutenants of the Royal Artillery, all of whom went through a course of preliminary training at the Magnetic Observatory, Trinity College, Dublin, under the superintendence of Professor Lloyd; in fact, the scientific supervision of the scheme was left in great measure in Lloyd's hands.

The first three officers invited to join the scheme were: the late Lieutenant General Sir J. H. Lefroy, the late Lieutenant-General F. M. Eardley Wilmot and Major-General C. J. B. Riddell. These were soon followed by the late General W. J. Smythe, Lieutenant-General C. Younghusband, Major-General H. Clerk, and the late Lieutenant-Colonel H. F. Strange. The naval expedition was placed under the command of Captain (afterwards Sir James Clark) Ross.

The observatories began their work in 1840. The first publication was a 4to. volume of observations on days of unusual magnetic disturbance, published in 1843, which was followed by a second, on the same subject, in 1851.

The subsequent publications are dated as follows:—

Toronto	to 1842, Vol. I.	1845.
"	to 1845, Vol. II.	1853.
"	to 1847, Vol. III.	1857.
"	The observations from 1848 to 1853 remain in MS.	
St. Helena..................	to 1843, Vol. I.	1850.
"	to 1849, Vol. II.	1860.
Cape of Good Hope, magnetic..	to 1846, Vol. I.	1851.
" " meteorological	to 1848, Vol. II.	1880.
Hobarton, Tasmania..........	to 1842, Vol. I.	1850.
" "	" Vol. II.	1852.
" "	" Vol. III.	1853.

Sir E. Sabine was enabled to complete this enormous amount of work by the fact that for upwards of twenty years a clerical establishment was maintained by the War Office at Woolwich, under his own special control.

From these official publications, we pass to Magnetic Surveys. Sir E. Sabine lived to complete in fifteen "Contributions" to the 'Philosophical Transactions,' a gigantic work undertaken by him, namely, a survey of the general Distribution of Magnetism over the Globe at this epoch. Several of these appeared after he had lost the aid of his establishment of clerks, the last in 1876. In these are to be found every observation of any authority, taken by sea or land, since 1818, or thereabouts, arranged in zones of 5° and 10° of latitude, and taken in the order of longitude eastward from Greenwich round the globe. All the results of Arctic and Antarctic voyages, and special expeditions were utilised. Everything that he could glean from Russian, German, American and other foreign sources, much of which is not accessible in any other form; and these were accompanied by maps prepared for him in the Hydrographical Department of the Admiralty, under the supervision of Captain (subsequently Sir) Frederick Evans, R.N. It may be safely said that had Sir E. Sabine

done nothing but collect, compile, and discuss this vast mass of material, he would have rendered a service to science, which would make his name live as long as Halley's, but it forms only a part of his life labour. Communications to this Society and the Philosophical Magazines, on some subject of the moment, were always flowing from his pen. His addresses to this Society and to the British Association, as President, must not be forgotten. Our Catalogue down to 1874 contains 101 titles of papers by him, and his activity did not cease with that year.

Sir Edward's military honours have been already enumerated, as well as the fact of his temporary service as Secretary of this Society, 1827–29. His subsequent appointments were as follows:—In 1839 he was elected General Secretary of the British Association, a laborious office which he continued to hold for twenty years, with the single exception of the year 1852, when he exchanged the Secretaryship for the Presidential Chair, at the first meeting at Belfast.

In 1846 he was elected Foreign Secretary of this Society, in 1857, its Treasurer, and finally, four years later, he was chosen President, an office which he held till 1871.

In 1821 he received the Copley Medal; in 1826 the Lalande Medal of the Institute of France, and in 1849 one of our Royal Medals. Of foreign orders he held that of *pour le Mérite* from Prussia, SS. Maurice and Lazarus from Italy, and the Rose from Brazil.

Turning to another view of his character, it may be said that Sir Edward's scientific capabilities were heightened by his social qualities. His grace of manner, his cheerful voice, and his brightness of aspect impressed all who came within his influence. A Fellow of the Society, who had travelled up from Manchester to be present at one of the Conversaziones, once remarked, "It is worth all the time and trouble to see, on arrival, the President's smile."

In the year 1876, his scientific activity came to an end. In 1879 he lost his wife, who for more than half a century had found her chief happiness in placing at the service of his scientific investigations the best efforts of a mind and a memory such as rarely have been given to any woman.

He himself finally passed away, at Richmond, June 26th, 1883, at the patriarchal age of 94 years 8 months. He was buried quietly at Tewin, Herts, in the vault belonging to his family, and beside the remains of his wife.

Sir Edward left no issue, and the very name of Sabine in the direct line of his family has become almost extinct, for his only surviving nephew on the male side, the late Admiral Sir Thomas Sabine Pasley, K.C.B., had taken the additional name of Pasley.

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END OF FIFTY-FIRST VOLUME.

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